RAILWAY LOCOMOTION,

AND

STEAM NAVIGATION:

Their Principles and Practice.

BY JOHN CURR,

OF NEW SOUTH WALES.

"Sapientem nihil admirari præter veritatem."

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SYNOPSIS OF THE CONTENTS.

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Dr. Hutton's errors as to force demonstrated. Emerson's and Hutton's Principles compared and tested by experiment, and to the errors of the latter as taught at the Royal Military Academy is attributed the decline of Science in Great Britain during the present century.

AN

ADDRESS TO THE READER.

Some months since it was comtemplated by influential parties in New South Wales to establish a railway from Sydney to Goulburne, the distance, as the crow flies, being about one hundred miles. A provisional committee was appointed to ascertain the probable income and expenditure, and engineers were invited to communicate with them, as to the practicability of the project and the best mode of carrying it into effect.

Although thirty-two years had passed since I relinquished that profession, I was induced to commence an examination of the principles of steam conveyance on railways, the result of which was laid before the committee in different papers, and forms the matter of the following pages, in so far as they relate to that subject.

That those papers were considered valueless and consigned to the shelf of oblivion, was evident

from their subsequent reports; but knowing them entitled to consideration, they were put in the form in which they now appear, with the intention of being made an eleemosynary tribute to the British government for the benefit of the British nation, if by chance their usefulness or value should be detected.

About the time of their being complete, H.M.'s sloop of war "Inflexible," J. C. Hoseason, commander, came to an anchor in the harbour of Port Jackson, and the newspapers reported in a tone which had the appearance of authority, some particulars of the engine, the proportion of its parts, and the speed of the vessel.

Her graceful figure excited the admiration of the multitude, and amongst the crowds who flocked to her decks I made a cursory inspection of the engine, in order to see by what inconceptible progress of mechanical ingenuity the power of the machine, as described, could be compressed into the small compass of 375 horses.

The result of that inspection was a suspicion that steam navigation, notwithstanding the thirty-five years which have expired since its importation across the Atlantic, is no better understood than on the day of its being landed; and which by a careful examination of two volumes and two odd numbers of the Mechanics' Magazine, made in order to ascertain the present actual and scientific knowledge of English engineers on the subject,

was fully confirmed. In consequence, the above named charitable intention has been abandoned, and the sister subjects of steam navigation and locomotion on land, are now supplicants under the same envelope, not for public favour or patronage, but for the strictly impartial and critical examination which, from the magnitude and importance of the subjects, they deserve.

The result of such change of purpose is, that of having quitted a peaceful homestead in the fair clime of Australia, and these lines are being penned on board the ship "St. George," bound from Sydney to London, so that in the monotony of passing over 16,000 miles of the hungry ocean, where all above the horizon is interminable blue, and all below differing only in shade and extent, an occasional excursion from the straight forward path becoming an essay on mechanical subjects, may in this address find pardon from the reader.

The value of a picture rests as much on its being the undoubted product of the hand of the master whose manner it bears, as on its own particular excellence, but it should be otherwise with the abased, unclassical, matter of fact subject of mechanics; the errors of the artist may be subdued by originality of colour, so that disproportion is ingeniously concealed from all but the scientific inspector of the painted canvass, and it may be the defect, by serving to prove the originality, enhances the marketable value of the picture.

The same courtesy, it seems, has been extended to the mechanical philosopher, for the great name of Dr. Hutton, prefixed to a course of mathematics, has propagated errors so manifold and atrocious on hydrostatics, the force and resistance of fluids, power and velocity, that before the reader be in a condition to examine the matter which follows, his mathematics must be disimbued of the acquirements of the school or academy.

Mr. Babbage in his reflections on the decline of science in England observes, "that a country eminently distinguished for its mechanical and manufacturing ingenuity should be indifferent to the progress of inquiries, which form the highest departments of that knowledge on whose elementary truths its wealth and rank depend, is a fact which is well deserving the attention of those who shall inquire into the causes that influence the progress of nations."

Believing Mr. Babbage to have justly estimated the quantity of science at the disposal of English engineers, it is difficult to conceive by what possible intervention of ingenuity, a country so circumscribed in the extent of its scientific acquirements, should deserve to be distinguished for its mechanical works, except by the power of great capital and industry; nevertheless I agree with him sincerely as to the utter disregard of engineers to the elementary truths he describes, and to such indifference or ignorance, for in the case before us they

are interchangeable terms, will herein be ascribed, as will afterwards be proved, the almost universal disagreement between theory and practice.

Besides the assertion of Mr. Babbage, that of Mr. Scott Russell, at a meeting of the British Association, held at York in 1844, may be admitted as evidence (for it was uncontradicted) touching the science of English engineering respecting locomotion on railways. He said, "How much mechanical force is requisite to move a given weight of train along a given gradient at a given speed, was a question of which the solution was essential to sound engineering; but the profession had long felt they were not in possession of sufficient data to determine the question."—(*Mech. Mag.*, vol. xli., p. 281.)

This is an admission in unequivocal terms that after fifteen years of experience, the body of civil engineers, with the help of the professors of mathematics at the universities and at Woolwich, have not been able to solve the simple problem, that they have so long stumbled at this *pons assinorum* in mechanics, and at length their mouthpiece, Mr. Scott Russell, procláims the data insufficient!! Let me ask when, whence, and by whom, the data are to be supplied or expected,—are we to wait until the iron-horse shall have been perfected in the English tongue, or do we wait for an April shower to supply the essentials of sound railway engineering?

I make answer, the iron-horse has long since spoken in intelligible terms—the desideratà are furnished in the following pages, obtained from no other data than such as the subject matter has prompted, and when this shall have been established, it will add one extra proof of want of science and defective engineering education.

Hutton's mathematics are the base of the mechanic's education, both civil and military; this may be fairly assumed, because he and Emerson are so opposed to each other that their co-existence under the same roof would be an outrage against common reason, and unbeseeming the great nation which nourished and gave them birth. Hence to establish a theory differing from that of Hutton, in respect to the subjects before mentioned, renders necessary a development of his mathematical vagaries, a task the more easy because his proofs seldom extend beyond that of bare unsupported assertion, or mystified deductions from false premises.

Undaunted, then, at the humiliating task before me, and although "fame will not protect the man of science from famine," I should have been as content at the age of sixty-four, with a slab hut in the pure air of New Holland, as in stepping out of obscurity, the sole and unsupported champion of that which is, and will ere long be proved the truth. Every attempt hitherto made to establish it through the medium of periodicals or newspapers, has been

smothered by the all-imposing name of Dr. Hutton; but, however great his merit as a schoolman, it is forty-five years since I found it essential to sound engineering to expel his course of mathematics from the workshop. Yet are his errors still garnishing the works expressly compiled for the use of the millwright, engineer, and ironfounder, so that, in short, Hutton's theories are taken upon trust as unquestionable truths; and to this cause may be attributed the little share of science possessed by modern mechanics, as asserted by Mr. Babbage and fully established by the avouchment of Mr. Scott Russell.

The grave charges thus insinuated will be proved by exhibiting Dr. Hutton's errors, and thence may be inferred the incompetence of our professors of mathematics, to apply that science to actual mechanical works wherein matter in motion is the agent, and whence may be assumed, the indifference of practical engineers to mathematical demonstration, as the natural result of ever finding theory and practice in discord. When this shall have been accomplished, the reader will be at liberty to form his own opinion as to the influence on a nation's weal, which may be expected from such revisal, and a theory established which no length of time can render more true as to the actual power required, to produce any desired effect on any level railway or gradient.

The principles of steam navigation will be also examined, and, as in the former case, no escape of time can render that more true to-morrow than it is to-day, so in the case now mentioned, a mode of propelling a vessel by steam through the water will be proposed, beyond which, so far as principle is concerned, no human ingenuity shall ever improve upon or render it more perfect. However presumptuous or over-assuming such language may appear, a careful reading is only asked to remove such impression.

It has been so often said that knowledge is power, the truism is worn threadbare and its pith exhausted; knowledge in these times is rather wealth, and wealth may be defined legal standard gold.

Gold, then, the measure of talent and man's happiness—the summit of hope and centre of earthly labour—has been prostrated by the million at the shrill call of the railway whistle; lines have succeeded to lines with such relentless anxiety, that the mechanic or mathematician, during the intermission of the fevered pulse, has not found leisure for suspense or a moment for doubt. The spirit of speculation and the lust of gain have staked out so much work to be dispatched in such breathless haste, that the body corporate has been in danger of expiring, not from inanition, but for want of fresh matter to supply the indomitable appetite for

other and more extended lines, and in the midst of such phrensy, railways have in all seriousness, become of great national importance.

In this late stage of the proceedings I stand forward to grapple with the subject, and although for the time above said I have been out of the mechanical profession, my leisure has been assiduously devoted to the separation of truth from error, so that theory and practice may ever be in agreement. Such then are the pretensions on which the author claims to stand before the public, but of their justice the reader will be the judge, and the matter which follows the evidence.

With a name scarcely remembered by the engineering world, occasional egotism may be taken as pardonable self defence, and that the author is not to be supposed a mere man of theory he may say, that shortly after the expiration of Mr. Watt's patent and at 18 years of age he was entrusted solely with the engineering department, both as to the plans and their execution, of what was then considered a rather extensive establishment in Sheffield Park, belonging to his father, and in which profession he continued about 16 years.

Whatever experience he might have gained in that time may be supposed to have little weight with the reader of the present day,—let it be so, but there are circumstances connected with his quitting the profession, the detail of which may be of some interest as a matter of history, for although

he may not before have placed his small talent in array against the united body of English engineers, his cause and that of justice seems to require that he should state the particular case which induced him to relinquish a profession, to which he had most ardently devoted his time and best energies, as on that occasion he was placed in opposition to three engineers, whose names will be afterwards stated, and who from their general repute may be taken as a fair specimen of the united engineering talent of the year 1815.

The case is as follows, Simon Wilkin, Esq., of Cossey, near Norwich, sent him (the author hereof) plans of a steam engine drawn by a Mr. Dodd, or Dodds, of Newcastle-upon-Tyne, intended to be put on board a vessel then being built, (the Orwell) and to trade between Harwich and Ipswich. Besides a multitude of minor defects in the plan, there were two of such magnitude that they were returned to Mr. Wilkin, and a reason given for having done so, which was that the engineer had not left space for the crank to revolve, so that if it had been so constructed it could not have made one revolution, and second that the air pump top, being in part below the cylinder bottom, and within about two inches of it, the air pump bucket, could not be put in its proper place, or extracted when required to be repacked, without the removal of the cylinder. Mr. Wilkin afterwards requested to be furnished with the plan of a proper engine

of ten horse power, and an estimate, according to which a contract was entered into and the engine put on board the vessel. When complete, the speed of the vessel being only five miles an hour—the power of the engine was doubted,—Mr. W. offered a compromise which was rejected, so that he sent to London by advice of counsel for three engineers whose names were Maudesley, Donkin, and Galloway, in order as was understood, that he might have sufficient weight of evidence to decide the disputed point.

The three parties just named continued during six days in one week, the examination of the principle of the engine, which was entirely new to them, as it would have been to any other parties whose services might have been called into requisition. On the sixth day Mr. Galloway was called to London on business, and the survey was continued by the remaining two, for three days in the following week, when they gave a report that the engine instead of being of ten horse power was only of five. The cause assigned for their decision being, that the engine having two cylinders, the connecting rods applied to cranks at right angles, and without the accompaniment of a fly-wheel, had no more power than one cylinder, for it was like two men turning a grindstone, and they had no more power than one!! In what school they acquired such notions of mechanics it is not for me to say, but such is the substance of their report as

expressed to myself by the lips of Mr. Donkin. A second compromise was offered and rejected, because by the aid of a dynamometer borrowed for the occasion of the Earl of Roseberry, the actual power of the engine had been proved to exceed that of ten horse power in a manner so conclusive, that an apprentice of six months standing might have known it.

Mr. Wilkin, acting under the natural impression that the contract had not been fulfilled, caused the vessel to be taken from her moorings in the night, and the engine to be ejected. Mr. Maudsley replaced it by an engine of twelve horse power, which when complete could move the vessel at only one-half the speed of the ejected engine, which as he had said was of only five horse power.

I, with the concurrence of my then partner, Henry Aggs, Esq., of Bruce Grove, Tottenham, built a vessel to receive the ejected engine, (the Defiance) which afterwards ran a race on challenge from near London Bridge to Gravesend, against the Margery, and beat her by one-half the distance. On another occasion, the Defiance, in consequence of an affront offered by the Prince Regent, at Margate, issued through the town a printed placard, saying that she would lay at her moorings until the Prince Regent was under full way, that she would pursue and steam round her, and leave her miles astern in the first hour, all of which was accomplished to the very letter, and in a short time

afterwards every new steamer which appeared on the Thames was supplied with an engine on the same principle as that of the Defiance, and of which the writer hereof is the inventor.

Trusting to memory he believes the engine of the Margery was of 24 horse power, and that of the Prince Regent, somewhat more, and from his determined perseverance in carrying out the invention, the advantages of which ought to be obvious, he claims the merit as well of the double engine, or two engines in one, as in general use in steam vessels, as of that due to the invention in its after application in railway locomotion.

The result was that finding ignorance silvered over with plausible appearances was of more weight than sound reason, that in a court of justice the weighty names above stated would have ground the inventor's fair claims into fritters, and finding that he must repossess himself of the ejected engine, before he could prove its power so as to become legal evidence, and that each party must consequently be released from the contract, he determined to quit the profession, and did so within a short time afterwards.

Another instance may be adduced of his not being a mere theorist, which is that some time previous to quitting his profession, application was made to him by an engineer to put a new end to an old cast iron boiler,—he refused to undertake it, telling the party requiring it what would ensue.

The party, however, still held his own opinion,—the work was done by another, and in some short time afterwards the new end separated from the boiler, that it is burst, and 16 lives were lost.

Besides the investigation of first principles which it will be necessary to make, we shall have occasion to examine with scrupulous vigilance, actual cases in practice and experiment on the large scale, for experiments when taken on apparent results are often as deceptive as the most false theory, and always require the exercise of the mind, to remove the impressions made on the visual organs, before the truth appears. As to railways we may discover deficiencies in the mode of carrying out the works, which although apparently insignificant are most material as regards wear and tear of the machinery and the safety of life and property.

With respect to steam navigation a recent instance of malproportion of the parts of the engine will be minutely investigated, and it will be shown that when the cost and weight of the engine was that of 1,500 horses, and the consumption of fuel that of 1,000 horses, its actual power did not exceed that of 300 horses.

The mode whereby this will be established is by reference to the particulars of the machine as compared with the effect produced,—next, by a comparison between the vessel referred to and the Inflexible, both as to power and resistance,—and lastly, the deficiency of power and consequently of

effect will be traced to the present highly fashionable, but most delusive theory of working the steam expansively. Such investigation is the more necessary, because the error seems to be gradually creeping, though on a diminished scale, into Her Majesty's navy, and I venture to say the exposition of the cause of failure will necessarily raise an enquiry of the deepest interest to the natural philosopher.

It would be mere mock bashfulness to endeavour to conceal it, that I have a desire, which has long been kept under, of rendering to mankind the small service my mean abilities, for such they are, will afford. The little I have acquired has been by unwearied study, often continued without intermission through days and nights, for by such exertion only are nature's laws to be unveiled. To this end, a continuous unbroken chain of reasoning is essential, the terminus of which should be, when the thing sought has been either found or proved impossible; for thus only is confidence acquired or rendered defensible.

The author of these pages has not been contented with a one-sided view of the case, nor have his decisions been made in precipitous haste, as too often happens with mechanics whose ingenuity is enrolled at the patent office. He however has determined to reserve within his own bosom two inventions, the one relating to railways, the other to steam navigation, which in due time will be offered to the British government, and if the pre-

mium asked be great, the terms and conditions on which it will be subject to being paid are such, that there is a possibility of the proposal being taken into consideration. Much will depend on the reception this book may receive from the public; and, as from the public I desire no favour, the tone to be assumed will be that which appears best calculated to provoke enquiry, and exposure of error, if such there be.

Had the author, in all honesty of purpose, cast these pages and the two inventions before the British government, they would have mouldered in the dust until destroyed by vermin; for the selfishness of our common nature would suggest, that if they were of value, I and every other person would have endeavoured to find a purchaser at their marketable price. Another course is nevertheless open to an inventor; but invention of late years has been of such a prolific nature, that it would require seven years to examine the rolls of the patent office, so as to draw a specification, when less than as many weeks may have sufficed to produce the matter to be patented. Moreover, I am willing to allow our engineers both the will and the way to harrass an inventor, so that he may be deprived of his fair deserts, or driven to seek redress through a doubtful suit at law; but which is a course not congenial to the ideas I have formed of the value of life's labour or measure of its happiness.

Should the proposal to the government be disregarded, my services will then be offered to a neighbouring state, in which I may still find in existence a long established claim as a mechanic, although I have not visited that country in person.

Both subjects, it is hoped, will be treated in a manner that mechanics may henceforward be less guided by rule of opinion, which is but a second edition of rule of thumb. Long prevalent error is too prone to pass as registered truth; but prescriptive authority is no longer evidence where its usurpation has been detected; and thus it is hoped the theories of Hutton, and such as are following in his wake, are doomed to banishment to the land of fables. When this shall have been accomplished, our country may deserve to scour the ocean in times of war, and supply the world with her industry in peace. Chemistry, astronomy, and the occult sciences, have had their full share of learned patronage, whilst the principles of mechanics, doggedly refusing to resign their matter of fact propensities, have been uninviting to the thirst of the speculative enthusiast. Our mechanics' institutes have made a flourish as to apparent utility, whilst their shelves are overstocked with absurdity and error; but the horizon of our commercial policy has been lately swept out, every corner has been cleared of the cobwebs which stood out in perspective—the growth of the last

half century,—and the subversion of these frail bulwarks may be a fit time to establish equitable laws to regulate matter in motion, so that after first principles shall have been established and settled, theory and practice will necessarily agree, and each hold its respective place in social intercourse; for the last at best is a slow and expensive preceptress, whilst the former aims at, and if the theory be good, arrives at the truth, if not at perfection.

With respect to the project named above, and which is styled the Great Southern and Western Railway, if it deserves further notice, I should say there is no prospect of its being carried out unless the capital be furnished from England. The colony is in gradual progression from its late sickness; and as her entire wealth depends on the precarious supply of natural herbage in the shape of wool, it is doubtful how long her apparent convalescence may continue. It has been the policy of the government to reserve in its grants of land the minerals for the crown—coal, of which a chartered company enjoys the exclusive privilege of supplying the colony through the length and breadth of the located districts, being an exception. Neither climate nor soil is peculiarly adapted for raising agricultural produce on a scale to

exceed the requirements of the colony, so that the reservation of minerals operates as a check on the profitable employment of capital. The colony is therefore dependant on the flockmaster for its progressive improvement; and if we contemplate the disease to which his stock is subject, the decrease in seasons of drought, and the further decrease consequent on that already named, and which arises from the necessity of boiling down cattle and sheep into tallow, to supply immediate necessities, the prospects af the colony are still precarious or equivocal. Under such view of the case it would be fearful to examine too severely the effect of dispensing a half million of English money, subject to interest at 12 or 14 per cent. The strictest economy is now being exercised in an endeavour to retrive past errors; but there are few who, having ten pounds to his credit with his banker, can say it is not engaged ten deep. There is a class possessed of capital, but such rate of interest would not induce them to expend it permanently, as it already produces an interest at a twofold rate. The climate of Australia is not supremely adapted to active industry; and when credit is cheapened, as it would necessarily be were England to supply the requisite capital for the railway, there is reason to fear the speculative spirit which so recently threatened to overwhelm the monetary establishments of the colony might be revived, for the spirit of speculation is not sub-

dued, but merely dormant for want of confidence and credit.

The pecuniary advantages then to arise from railways, become in degree secondary to other considerations; but as the colony may be considered without roads, the soil is absolutely valueless except for its natural grass. About £40,000 is said to be subscribed in the colony; but it is doubtful whether a tithe of that sum could be conveniently paid down; nevertheless, as the expense of a substantial wooden railway, as will be afterwards described, would exceed that of a good road in a very limited degree, it may deserve consideration from the government, whether the necessary expenditure would not be speedily repaid by the increased sale of government land. If, at the same time, the manufacture of iron were commenced, amidst interminable forests of the finest timber, valueless except for the purpose of being reduced into charcoal, such manufacture would, as time advances, become an adjunct to the present staple produce of the colony, so that her welfare would not be solely dependant on rain from the heavens.

The value of charcoal iron is well known in England, and it commands a price accordingly; the charcoal of Australia would be of nearly twice the specific gravity of that usually produced from top and lop in England, so that it is possible English capital might gradually be transplanted there, and

thus an expenditure on a railway, probably not exceeding £100,000, would in a very few years raise an income, which would gradually extend the railway through the chief located districts. I am enabled to say, with considerable confidence, that a railway from Sydney to Goulburn, with all appurtenances, would cost about £2,000 a mile; and from opinions given by parties competent to form a judgment, I estimate that the saving of tallow, by the conveyance of cattle on the railway to Sydney, would repay all the expense of working it, and 12 per cent. interest on the capital employed.

If such estimate be an approximation to truth, a railway on the progressive principle would be of more value to the colony than half a million at once thrown into its lap. By the mode proposed, railways would advance slowly, but beneficially; and it may be worth the experiment, whether the charcoal iron produced in the colony might not supersede the use of iron from Sweden or Russia, for the purpose of being converted into steel. If this be still problematic, it might at least preserve the reputation of the mother country, in sending to market an article called iron, but which foreigners find no better than "pot metal;" an article which inundates Sydney, and which it would be true economy to cast into the depths of the sea.

Such gradual progression as has been proposed, would not materially infringe on any established or vested interest at home or in the colony. To estimate the charges of freight of iron from Sydney

to London, taking wool at £12 a ton, and the space it occupies 60 cubic feet when pressed, and a ton of iron to occupy five cubic feet, we will first see what would be the freight of iron per ton, so that the ship owner receive the same amount on a joint cargo of iron and wool, as if the vessel were entirely loaded with wool at the above rate, and taking 40 cubic feet of water to weigh one ton.

First let the vessel be supposed capable of carrying 100 tons of actual weight of goods, and the water displaced thereby would be $100 \times 40 = 4{,}000$ cubic feet. Consequently that space would contain $\frac{4000}{60} = 66\frac{2}{3}$ tons of wool, which, at £12 per ton, the freight is $66\frac{2}{3} \times 12 =$ £800.

We will next enquire what is the greatest weight of iron which, being shipped together with wool, and at what rate of freight it should be charged, so that the freight on the whole cargo fulfils the conditions specified.

Let $x =$ the weight of iron, and $\frac{60}{5} = 12$, the ratio of the freight of wool to that of iron, consequently the freight of iron would be $\frac{12}{12} =$ £1 per ton.

Then $x + \frac{4000 - 5x}{60} = 100$,
and $60x + 4{,}000 - 5x = 6{,}000$,
or $55x = 2{,}000 \therefore x \frac{2000}{55} = 36\frac{4}{11}$ tons of iron, and consequently the weight of wool to complete the cargo would be $63\frac{7}{11}$ tons.

Proof.—	$36\frac{4}{11}$	tons of iron, occupying	$118\frac{9}{11}$	ft. at	£1	per ton	£36	7	$3\frac{3}{11}$
	$63\frac{7}{11}$	„ wool „	$3{,}818\frac{2}{11}$	„	12	„	763	12	$8\frac{8}{11}$
Total	100	Displacement by cargo	4,000	ft. Amt. of freight			£800	0	0

Such proportional quantities would be most to the interest of the ship owner and the public; for the cost per ton on the whole cargo is a minimum, and the amount of freight a maximum. But as wool alone would not be a fit cargo for any vessel, and as oil, tallow, bones, &c., are being taken as dead weight at from 10*s.* to £4 a ton, it would probably be found, were the calculation made on any such specific cargo, that the freight receivable on the whole would not exceed £6 per ton; and if this estimate be correct, the ship owner could not only afford to carry the iron for nothing, but to pay the shipper £10 per ton for the privilege of taking it, and still be as well remunerated as in case of a mixed cargo as just described.

Proof.—A mixed cargo of wool, bones, &c., to average £6 per ton $\frac{800}{2}$					£400 0 0
$66\frac{7}{11}$ tons of wool at £12 . . .	763	12	8	repaying from	
which ($36\frac{4}{11}$ „ iron „ 10) . . .	363	12	8		
there remains as produce of the freight	£400	0	0		

INTRODUCTION.

FORCE AND RESISTANCE OF FLUIDS.

THE value of the following pages, in so far as they relate to steam navigation, and the resistance of the atmosphere to the progress of a train on a railway, will be estimated according to the soundness of the reasoning to be exercised in the investigation of the principles which govern what is usually termed power or force, effect, resistance, and velocity of fluids. Mechanical philosophers have left the subject in great obscurity, by using such terms in their general sense and without definition, so as to convey a distinct idea of the particular case implied or contemplated, insomuch that theory and practice find themselves as much at loggerheads as if they were offshoots of different principles. For example:—the resistance of a fluid to a body in motion through it, is said by all mathematicians to be as the square of the velocity, a theory equally true and false; that is, it is true as a particular rule, but not so as a general rule: for if the resist-

ance in a given distance be as the square of the velocity, it is when the time is given as the cube, as will afterwards be made evident.

Hence the necessity of defining force and resistance, so that the sense may be limited to the particular case proposed or contemplated, which I purpose doing as follows :—Power or force admits of three distinct limitations, viz., the force during a given time, the force in passing a given space or distance, and the force taken, measured, or considered, irrespective of either, which last described will be herein-afterwards termed the module of force, and the two former the force or whole force, in a given time or distance, as the case may be.

Resistance admits of three similar limitations, and will be termed accordingly ; but if the subject about to be examined were intended to be treated in a manner purely elementary, a further subdivision might be necessary.

The module of force requires a more explicit definition, which may be given as follows :—the velocity of a fluid in any indefinite time is equal to twice the space fallen by a heavy body to acquire that velocity ; for if the time be half a second, the body will have fallen 4 ft., and acquired a velocity of 8 ft. per half second ; and if the time be one second, the body will have fallen 16 ft., and acquired a velocity of 32 ft. per second, and so of any other particular time ;—consequently the velocity of a fluid in any indeterminate time, or in any or

every particle (considered indivisable) of time, is twice the altitude of the fluid requisite to produce its velocity; whence, as the velocity of the fluid represents and is equal to the quantity of fluid in motion, its force is equal to twice the altitude; and the force of a fluid in motion against a plane at rest, as intended to be expressed by its module, is equal to the column of the fluid whose base is the section of the stream,—and altitude twice the space descended by a heavy body to acquire the velocity of the fluid. The module of resistance differs from that of force, as will afterwards appear.

To propose a theory which would be in agreement with the limitations given, would be a simple process,—to prove its truth would be equally so; but it is due to Dr. Hutton and the professors who have succeeded him, as well as to the mechanical world, not to attempt the establishment of a fresh theory, without, at the same time, proving that of Hutton erroneous.

This will be accomplished in the following manner:—first, to advance that which is proposed as the true theory, to prove its truth, to compare it with those of Hutton and Emerson, and lastly with actual experiment made by parties usually quoted as good authority by mechanical or mathematical writers.

In conformity with the design just described, I say,—The whole force in any given time of a fluid in motion against a plane at rest, is the complicate

of the quantity of fluid striking the plane in that time, its density, the area of the plane, and the altitude a heavy body falls to acquire the velocity of the fluid; for the velocity represents the quantity of fluid which strikes the plane in the time, and the altitude represents the force or pressure of each particle on striking it.

If, then, we make a the altitude of a column of the fluid in feet requisite to produce the velocity, v, in feet per second (omitting the area of the plane and density of the fluid as common terms in any case of comparison), the force of the fluid per second is $a\ v$. And as an example :—If the velocity of the fluid be 32 ft. per second, the altitude due to the velocity (to use an expression of Dr. Hutton) is 16 ft., and the force per second, therefore, is $a\ v = 16 \times 32 = 512$.

The above theory needs no further proof, as it agrees with all knowledge, practical and theoretic, of force (Dr. Hutton's excepted), which is, that in all uniform motion, generated by the force of gravity, the quantity of force or the momentum in any given time is the complicate of the quantity of matter, and the space it has fallen during that time.

Dr. Hutton's theory is (see Ed. 6, p. 257,) as follows.

PROPOSITION LXXVIII.

"*The real resistance to a plane, by a fluid acting in a direction perpendicular to its face, is equal to the weight of a column of the fluid, whose base is the plane, and altitude equal to that which is due to the velocity of the motion, or through which a heavy body must fall to acquire that velocity.*"

THIS proposition appears to refer rather to the resistance of a fluid to a plane in motion through it, than to the fluid's force when in motion against a plane; but as in Hutton's demonstration, if it may be so termed, the force and resistance are made equal, the same theory is applicable to both cases.

The author has not resorted to this mode from choice, but from necessity, for Hutton invariably mixes force and resistance together, as if they were universally equal,—a fact not proved, nor never will be, for its truth rests solely on his own unsupported assertion. This question will be raised and disposed of afterwards; but before we are in a situation to examine the proposition, it will be well to deprive it of its ambiguity, as it seems rather an enigma to be unravelled, than a proposition deserving a place in a course of mathematics.

"The real resistance to a plane by a fluid acting," &c., seems to imply that the fluid is the acting matter or agent, and consequently the plane the patient; if so, the resistance would not be *to* a plane, but *by* a plane; and the resistance so taken or understood would not be *by* a fluid but *to* a fluid: on the other hand, if the plane be taken as the agent the fluid would be the patient, when it could not be considered as *acting* in a direction, but as being *acted* on. Then in what way soever the proposition is construed, it is absolutely unintelligible jargon, and would have been passed over without comment, were it not a specimen of the doctor's usual and ingenious mode of settling disputed questions, for he and his prototype Emerson differ on the point precisely as 1 to 2.

Emerson has mathematically demonstrated (2nd ed. quarto, Princp. Mec. Prop. 107 and 108, pp. 152 154) that the force of a fluid in motion against a plane at rest, is twice the resistance in the contrary case, and it is confirmed by experiments; but Hutton contrived to establish his new-fangled theory by the single unsupported, but often repeated, assertion, "that it is evident."

An evident mistake it will soon appear, and if it continue to this day unabated, as I incline to suspect, the professor's chair for the last forty years must have been a very soft and soporific cushion.

Hutton's solution of the proposition is that the

real force of a fluid in motion against a plane at rest, is $\frac{v^2}{4g}$, — v being the velocity of the fluid in feet per second, and g the space fallen by a heavy body in one second. Then the real force of a fluid in motion against a plane at rest, or the whole force, as expressed in the demonstration, taking v, the velocity of the fluid, at 32 ft. and g at 16 ft., is $= \frac{v^2}{4g} = \frac{32^2}{64} = 16.$

The difference then between the theory now attempted to be established and that of Dr. Hutton is as 512 to 16; the force by Hutton being as the square of the velocity, and by that proposed as the cube, or the difference in the case, is as 32 to 1.

We will now compare the force of fluids at different velocities, and their effect according to Hutton's theory and that now proposed to supersede it, the two cases being taken as follows:—

	Act. of fluid.		To give a velocity.	The effect is.	
Case	1.—16 ft.	...	32 ft. per sec.	$\frac{32^2}{64} = 16$	per Hutton's theory.
	2.—64 „	...	64 „	$\frac{64^2}{64} = 64$	

Hence when the quantity of fluid striking the plane is doubled, and the force or pressure with which the plane is struck quadrupled, the actual force employed is as the cube of the velocity, whilst the effect is (on inspection) as the square. But Hutton has said the force and effect are equal, consequently in whatever way the proposition is interpreted, it is a self-demonstrated error.

The force and effect by the proposed theory are equal, and as follows :—

		Act.		Vel.	
Case 1.—The force and effect		16	×	32	= 512.
2.—	„ „	64	×	64	= 4096.

That is the force as well as the effect in equal times are as the cube of the velocity, the quantity of fluid in case 2, is double that in case 1, and the the pressure of each particle fourfold; therefore the theory agrees with all knowledge actual and otherwise of force and effect, and so is obviously true; but if Hutton's were so, it would upset all actual knowledge of force and effect, and Newton must be summoned from the shades to establish a fresh system of the laws of gravitation.

Hence the resistance to a body moving through a fluid, be it a ship or a paddle, is in theory as the cube of the velocity when the time is given, and the power required to overcome that resistance is in the same proportion; but when the distances passed over are given, the time of acting will be diminished inversely as the velocity is increased, consequently in equal distances the power and resistance will be as the square; for example, when in case 2 the velocity is double that in case 1, they would be 512 and ($\frac{4096}{2}=$) 2048, which are as the square of the velocity.

That no doubt may remain of Hutton's errors his theory will now be compared with actual experiment. Hutton in proving proposition 78, says—" The force of the fluid in motion is equal to

the weight or pressure which generates that motion, and this is equal to the weight or pressure of a column of the fluid, whose base is the area of the plane, and its altitude that which is due to the velocity."

The plain obvious sense of which is, that the force of a fluid in motion, is equal to the weight of a column of the fluid at rest, which produced that motion; whence it is to be inferred, that a fluid in motion has no more force than a fluid at rest!!! and Hutton consequently makes the whole force or resistance $\frac{v^2}{4g}$, or the altitude of the fluid!!!

A simple note of admiration will meet the case as fully as the soundest reasoning, for the experiment of John Banks (Treat. on Mills, p. 114) shows, that when the weight of the perpendicular column of water is 25, the force when in motion, as ascertained by a steelyard, is 48; therefore if 4 per cent. be allowed for friction or error in the observation, his experiment agrees precisely with the theory of Emerson, that the force of a fluid in motion is equal to a column of the fluid whose base is the section of the stream; and its length, twice the height descended by a falling body, to acquire the velocity of the fluid; consequently, Banks' experiment differs from Hutton's theory just as 2 to 1.

The theory of Hutton is still to be compared with the experiments of Mr. Rous, Dr. Lind, and Colonel Beaufoy (Tredgold on strength of iron), as

to the force of the wind. The density of the air at a mean, according to Hutton's table, is $1\frac{2}{9}$ ounce (= ·07639 lbs.) per cubic feet, and the force on a surface 1 foot square, when the velocity of the wind is 20 feet per second, is $1 \times {\cdot}07639 \times \frac{20^2}{64} = {\cdot}07639 \times {\cdot}6{\cdot}25 = {\cdot}4774$ lbs.

The force, by the experiments above quoted, when the velocity is the same, is ·925 lbs. If then we allow 3·9 per cent. for error in the mean density or in the observation, the experiment agrees with the theory of Emerson and that now proposed, and differs from that of Hutton in the same proportion as in the experiment by Banks.

Had I the talent or ingenuity, I might search for an excuse for the doctor; but all that can be said in his favour is, that his disproof of Emerson is merely declaratory, and unsupported by sound reason; and therefore, when he penned the proposition quoted above, he must have frequently drawn so freely, on his pupils' ignorance and credulity, as well as on that of his subordinates, as to assure himself, his usual bill would at sight be honoured, and without demurrage. But if an excuse is not to be found for the error, its cause is sufficiently tangible. Hutton has said, the force of a fluid in motion against a plane at rest, is equal to the resistance of a plane moving through a fluid at the same velocity. To this I say, the cases are not similar or parallel; for when the plane is in

motion through the fluid, effect is produced on the fluid, by its partaking of the velocity and force of the plane; but in the opposite case, the plane continues stationary, so that effect is not produced: but were the plane to recede on being struck by the fluid, (as in case of theunder shot water wheel), the force would be reduced one half; for the water would strike the wheel, not with its absolute velocity, but with the velocity taken relatively to that of the plane, and in such case the force on the plane would be equal to the effect produced, so that force and resistance would be equal. Hence it is manifest, that the force of a fluid in motion against a plane at rest, is to the force in the opposite case as 2 to 1; and hence is Hutton's error once more established, and Emerson's theory proved.

From the preceding it follows, that when the module of force or resistance of a fluid against a plane at rest, shall have been determined by experiment, as in the case of those of Banks, Mr. Rous, &c., the whole *effective* force or resistance in any given time, is the complicate of the module, and half the velocity or space passed over by the fluid in that time.

The necessity of the three distinctions or limitations being made, to render the subject intelligible, and to reduce the subject of force and resistance into such compass, that the practical mechanic may avail himself of the result, will be admit-

ted by all, unless the mathematician, who, for the sake of a pure geometrical or analytical process, and to pursue it in all the elegance of its integrity, shall object, that the subject has been reduced to the level of the unlettered mechanic. If so, it will be pleaded in abatement, that since the great talent of the nation, with its confusion of terms, left the subject in more than its original obscurity, the antagonistic principle, or plain common sense, has been brought into the field to meet it, and establish truth in such manner, that theory and practice may once more be reconciled, and in terms of agreement.

The concluding observation is, that when we estimate the resistance of the atmosphere to the progress of a train, the result will differ from that by Hutton's rule as 1 to 2; and when we estimate the resistance of a paddle or vessel, the difference will be as 32 to 1, when the velocity is 32; and as 64 to 1, if the speed of the vessel be 64 feet per second.

ON MOTION, FORCE, AND VELOCITY.

THIS subject will be examined as respects the force of impulse or percussion by falling bodies, as in case of pile driving. It may be proper to observe, that Emerson has made it *proportional* to the absolute velocity or space fallen ; and Hutton, after considerable shuffling and equivocation, has made it *equal* to the square of the (acquired) velocity. The subject particularly deserves examination, on account of the ridiculous position in which it has placed his disciples, Mr. Partington, of the London Institution, and Mr. Tredgold.

W. Emerson, the translator of Sir Isaac Newton's Principia, and the only mechanic of the past century, has given the relative momentum of a falling body as compared with the same at rest, and says—

Let b = body or quantity of matter.
F = accelerative force acting uniformly and equally on the body b.
v = velocity generated in b by the force F.
m = the motion generated in b (by some writers, as he says in his definitions, called the momentum).
s = space discribed by body b.
t = time of describing the space s.

Amongst other rules of proportion he makes *m proportional* to *b v*, that is, to the product of the quantity of matter and velocity.

Before proceeding in the examination, it will be proper to have a distinct perception of the term velocity, which, in the case before us, may be distinguished as absolute velocity and acquired velocity. Absolute velocity, according to the definitions of both Hutton and Emerson, is the space passed over by a falling body in respect to a body at rest, and consequently is equal to the space or distance the body has fallen; so that absolute velocity is taken or measured, irrespective of the time of falling.

Acquired velocity has not been especially defined by either writer, and therefore will be taken in the sense in which the term is universally used, viz., the velocity in respect to any particular time, usually in feet in the time of one second; or as the space in feet that would be passed by the body in one second, in case its volocity were to be continued uniformly, after having acquired that velocity per second.

I now say, the obvious plain reading of Emerson is, that *m*, the momentum generated in the body *b*, when it has been acted on by the force *F*, and has described the space *s*, whatever be the time in which it has described that space, is *proportional* to *b v*, or, to the product of the body and the *absolute* velocity; being equivalent to the product of the body and the space it has fallen, and which is

consequently, the weight of the body multiplied by the space it has fallen.

Dr. Hutton, treating on the same subject, says,

Put $b =$ any body or quantity of matter.
$f =$ the force constantly acting on it.
$t =$ the time of its acting.
$v =$ the velocity generated in the time t.
$s =$ the space described in that time.
$m =$ the momentum at the end of that time.

The plain reading of Hutton then is; m, the momentum at the end of the time t, when the body b has been acted on by the force f, and acquired a velocity v, in that time, and whatever space it may have described in that time, is proportional to $b\,v$, or to the weight of the body multiplied by the *acquired* velocity.

The acquired velocity being as the time, or as the square root of the absolute velocity, Hutton and Emerson consequently differ as $b\,v$ to $b\,v^2$, although each represents the momentum in the same symbols, or as proportional to $b\,v$.

The next point to be examined is the actual force of a falling body, coming in contact with any other that obstructs its motion, and which is termed the force of collision or percussion.

Emerson has given the proportion, but not the equality, as $b\,v$, that is, the momentum is proportional to the body multiplied by the space fallen.

Hutton has proceeded further, he says "a motive or a moving force is the power of an agent to produce motion, and it is equal or proportional to the momentum it will generate in any body, when acting either by percussion, or for a certain time as a permanent force." This is an obscure sentence, and must be examined in order to be intelligible. Does he intend to say that in both cases particularized the momentum is equal and proportional to to the motive force, or that in one case it is equal and in the other proportional,—if there be doubt the doctor shall have the benefit, and we will read the definition thus: if the force acts permanently as in uniform motion for a certain time, the momentum is *equal* to it, but if by percussion, the momentum is *proportional* to the motive force.

We will proceed to examine the doctrine delivered by Dr. Hutton (page 341) in "Practical Exercises Concerning Forces; with the Relation between them and the Time, Velocity, and Space described." He there says, "when a motive force happens to be concerned in the question, it may be proper to observe, that the motive force m of a body, is equal to $f\ q$, the product of the accelerative force and the quantity of matter in it q"—also "the momentum or quantity of motion in a moving body is $q\ v$, the product of the velocity and matter."

It is now to be observed, that the doctor first makes the momentum, proportional to the product of the quantity of matter and velocity,—afterwards

it would seem it might be proportional or it might be equal,—but at length he says in plain words, that the momentum is equal to the body multiplied into the velocity. He has not here said whether v, the velocity, represents the absolute or acquired velocity of the body, and I will therefore give the reader the benefit of choosing either, for various writers, for example, Mr. Partington and Mr. Tredgold, seem to interpret it differently. We will, however, follow the doctor to a practical case, when we shall see the capers he cuts and the agility he displays in endeavouring to extricate himself from the shaking quagmire on which he finds himself: viz., to shift the momentum from v, as given in his original theory, to v^2 in a practical case. We will now examine

PROBLEM VIII.

To determine the effects of Pile Engines.

"Belidor (he says) has given some theory of the effects of the pile engine, but it appears to be founded on an erroneous principle: he deduces it from the laws of the collision of bodies. But who does not perceive that the rules of collision suppose a free motion and a non-resisting medium? It cannot therefore be applied in the present case,

where a very great resistance is opposed to the pile by the ground. We shall therefore here endeavour to explain another theory of this machine."

"Since the percussion of the weight acts on the pile during the whole time the pile is penetrating and sinking in the earth, by each blow of the ram, during which time all its force is spent; it is manifest that the effect of the blow is of that nature, which requires the force of the blow to be estimated by the *square* of the velocity. But the square of the velocity acquired by the blow of the ram, is as the height it falls from; therefore the force of any blow will be as the height fallen through."

Here, then, the doctor agrees with the theory of Emerson, that the momentum is as the space fallen through, or as the absolute velocity, and has resigned his original theory given as $b\,v$ for that of $b\,v^2$, so that in fact, he could not know the distinction, except by name, between the one and the other.

We will now examine the apology for shifting from v in the theory, to v^2 in the practical case. It cannot be that because the pile sinks when struck by the ram, that the force becomes swollen from v to v^2, neither is the rule to be changed because the effects of a blow may vary. If the pile were hard and were not to sink, the ram would rebound to the height from which it falls, then the height is the measure of the force whether the pile sinks or stands firm; or the height is the measure of the

momentum, so that the sinking of the pile is no reason for increasing the momentum from v to v^2, for whether it be measured by the space fallen, or by the acquired velocity, the pile will sink alike; and if the doctor's rule were good his theory is bad, for he has said, the force of any blow is the height fallen through.

What then becomes of the taunting question, "who does not perceive that the rules of collision suppose a free motion and a non-resisting medium, and are therefore inapplicable when a very great resistance is opposed to the pile by the ground?"

It is like scattering dust over the minds of his pupils, for as he was measuring the momentum by the effect produced, it ought rather to be reduced from v to its square root, than to have been advanced in an opposite direction.

The doctor afterwards proceeds to enquire "whether the business may be effected in a less time by a greater height of the machine, or whether there be any limit to the height, so as to produce the greatest effects in a given time; and having gone through an analytical process of reasoning, concludes, that on the whole it appears, the effect in any given time increases more and more as the height is increased."

The more and more the doctor advances, the deeper and deeper will he be found in the quagmire. For I say the power required to raise a given weight is as the height, and the force of the

blow, as the doctor has admitted, is as the height, therefore the height to which the ram is raised is a neutral consideration, and so the doctor in raising the question stultifies both himself and his pupils.

Bt he says there is no limit to the height, and the more the height is increased, more and more is the effect increased in a given time. To this I say, the power required to raise 1 lb. 1,000 ft. and 1,000 lbs. one foot is equal, and the momentum is equal, but the effect would differ according to circumstances. The resistance might be such that the larger weight would sink the pile by its gravity, and without any fall, whilst the small weight with its great velocity, would not overcome the vis inertia of the pile, but penetrate if soft, or rebound if hard. Therefore, the power required to raise the ram being in both cases the same; and the greater effect being produced by the larger weight, it follows that as a general rule, the greater the weight of the ram, the greater is the effect produced, by an equal power in the same time. As to the atmospheric resistance and the friction of the ram, if they deserve to be noticed, they would be in favour of the larger weight as 60 to 1, if estimated by the last velocity, but if according to actual resistance in a practical case, they would probably be in its favour as 1,000 to 1.

It may however be needful to observe, that the weight of a ram should always be proportionate to that of the pile, and the height to which

the ram is raised proportionate to the strength of the pile, and by such means only will the most lasting foundation be laid, and at the least expenditure of labour.

Having gone over in succession the doctor's shifty arguments, we now come to the catastrophe, to which his doctrine has lead the way.

Mr. Partington, of the London Institution, an evident disciple of the doctor, in his edition of "Ferguson's Lectures, *Improved and Adapted to the Present State of Science*," says the ram used for piling at Westminster bridge weighed one ton, and that its fall was 30 ft. In notes 10 and 42, doubtless being adapted to the present state of improved science, he says, "it is only necessary to add, that the momentum thus acquired is *exactly* equivalent to so much additional weight falling on the head of the pile." For additional weight let us substitute multiplicational, as it is not usual to add weight to time, space, or velocity, and the force of the ram used at Westminster bridge unless I mistake, would be exactly 30 tons.

According to Mr. Tredgold, whose theory we are about to examine, it would be exactly 1,920 tons, so that I merely at present enquire what further evidence is needed of the impoverished state of mechanical science.

We will examine Mr. Tredgold at some length, not only because he is a disciple of Hutton, but by the advertisement to the third edition of his Prac-

tical Essay on the strength of Cast-iron, it appears "its progress through the press was kindly superintended by the author's friend, Professor Barlow, of Woolwich, and that its correctness cannot therefore be reasonably questioned."

In treating on resistance to impulsion (p. 257) are the following rules: multiply the weight of the falling body in lbs. by the square of the velocity in feet per second, and if the height of the fall be given instead of the velocity of the falling body, then instead of multiplying by the square of the velocity, multiply by 64 times the height of the fall.

In this case I fear Mr. Tredgold's sarcastic quotation (p. 3), that "the stability of a building is inversely proportional to the science of the builder," will recoil where it would meet the least welcome.

I ask, why measure the velocity in feet per second, rather than in inches or yards, or what compact exists between nature's decrees and customary measurement, as established by man, to determine the momentum by velocity taken in feet per second, rather than in inches or yards.

If such customary measurement be used, and Hutton's rule followed, $\frac{v^2}{q} = \frac{32^2}{16} = 64$, where q is the space fallen in one second, and v the acquired velocity in falling one second; therefore, for each foot fallen, the falling weight is increased 64 fold,

as shown by Mr. Tredgold. But measure the space fallen in inches (192), the velocity is 384, and $\frac{384^2}{192} = 768$. Thus the momentum would be 768 times the inches fallen, or $768 \times 12 = 9{,}216$ times the feet fallen, instead of 64.

If this be not satisfactory, I ask by what prescription of nature's laws has the custom been established, of measuring the velocity of falling bodies in feet per second, and why not divide the minute into 120 equal parts or times, each equal to that, in which a pendulum about $9\frac{3}{4}$ inches long, will make one vibration. Then in one time or vibration a heavy body would fall 4 ft., and its acquired velocity would be 8 ft. per time or vibration, consequently $\frac{8^2}{4} = 16$; so in such case the momentum would be reduced from 64 to 16 times the feet fallen; and would nature's laws, or Dr. Hutton, allow a second to be divided into 100 equal parts, the momentum would be reduced to ·00016 times the feet fallen, for that would be equal to the square of the acquired velocity, per the one-hundredth part of a second.

When such absurdities are being taught, both in the mechanical world and in the higher departments of scientific investigation and educational study, it will cease to astonish the vulgar, that theory and practice have usually been in such determined opposition, or that Mr. X, or Mr. Y, is

stigmatised by public consent as a mere theorist, and branded on the back accordingly.

Besides Hutton's equivocations and shifts noted in examining this subject, it may be proper to observe, that he let Mr. Tredgold into his own quagmire, by having made *q v equal* to the momentum, instead of *proportional*. The difference between Mr. Partington and Mr. Tredgold seems to be, that the former was guided by Hutton's first established theory, and Mr. Tredgold by the last.

It would seem the force of a falling body can be determined only by experiment. Mr. Tredgold tried it (p. 99), and found the effect not to tally with the rule; but with all his acuteness the rule continued undoubted, for he promised at a future time to resume his experiments, probably supposing nature's laws out of joint at the time, or playing at cross purposes.

The foregoing subjects have been examined preparatory, and in order that he who has read them may be better prepared to understand those which follow. They are probably not the richest specimens adducible of Hutton's vagaries; but were we to construct a descending scale, they might nestle about *No.* 3. The author holds himself responsible, as well for the correctness of the strictures, as to further supporting them, should there arise need of it. It seems just possible a glimmering of enquiry may be aroused, whether reason deserve to hold the rein in mechanics, or whether

our railways and steam navigation are still to be subject to *rule of thumb*; in which case the political economist may do well to consider, whether decay of science be not the sure, but tardy, forerunner of a nation's downfall.

In taking leave of Hutton's mathematics, I may say, that, to his doctrine on hydrostatics might fairly be attributed, the bursting of steam engine boilers, the blowing up of public sewers, and the breaking down of brewer's vats; so that his course of mathematics is equally a national misfortune and disgrace. But to render him the tribute he deserves, as a school-man, in figures he was pre-eminent, but as a mechanic, pragmatic and powerless.

If, independently of absolute error, we note his constantly doubtful sentences, his confused ideas, his enigmatical propositions or corollaries, his drawing true deductions from false premises, his self contradictions, his suppositions on practical cases which in practice could not exist, and then draw a line between his great powers in one way, and his defects in the other, there might be little difficulty in deciding what benefit he conferred on the nation, when he ceased to flog the lazy gin-horse at Benwell coal mines.

RAILWAYS.

THE progress made in England and elsewhere, during the last few years, in the establishment of railways, has rendered the subject of universal interest, insomuch, that it is the duty of every one, to contribute towards their further development, the information he may have acquired.

To enlarge on their advantages, their tendency to break down monopoly, to equalize contending interests, and to give as, if by magic, a sort of omnipresence to persons and things, would be a waste of time; suffice it, they have advanced so far in England, their establishment throughout becomes a necessity, as otherwise, such places as are destitute of the prompt communication they afford, may be erased from the map or become valueless.

With these views, I shall endeavour to point out the best construction, both as to first cost, annual expenditure, duration, speed and safety, and also to devise means whereby a railway may be

carried out, so that its expense in case of limited traffic, be proportional to the income expected to arise from it.

To accomplish this, it is essential to determine accurately, what power is necessary to convey a given weight on a level railway at any speed,—the utmost rise that can be ascended by that power or any other,—the speed on ascending it, as well as on any lesser gradient, and also the maximum weight of train, that can be moved by any particular locomotive engine.

If the railway be level, the entire power of the engine is absorbed in friction, and in overcoming the resistance of the atmosphere,—the former is as the velocity of the train,—the latter is as the square or cube; and therefore in the first process, it is desirable to leave the atmospheric resistance out of consideration.

It may however be observed, that in case of high velocity, atmospheric resistance absorbs a very large portion of the entire power of the locomotive, and that if engineers in England have not already done so, the means of reducing the resistance is extremely simple.

Before we can estimate the amount of friction on any railway, it will be proper to establish a standard, which being adjusted to any particular case, shall also be applicable to every other. The friction on the rails, when the line of railway is straight, and the wheels cylindrical, is in theory

nothing, as may be geometrically demonstrated; so that in theory it will not deserve consideration, independantly of the friction on the axle. That such last mentioned friction will depend on the workmanship and material, might seem an obstacle to establishing a general rule; and if so, that the rule would require to be changed, according to the material used, and the quality of the workmanship; and it is therefore necessary to premise, that the standard of friction will be taken, as of fairly executed workmanship, the axle and bearings being of the usual materials; and as every day's wear is a remedy for indifferent workmanship, there seems no difficulty in establishing a rate of friction, which shall as truly represent the power required to overcome it, as if that power were employed in raising a weight, as adopted in fixing the standard of horse power of steam engines for general purposes. As the subject of friction will be more particularly examined in a subsequent chapter, in order to exhibit the errors of mechanics in respect to it, all that need be said at present is, that the perpendicular friction on the axle, as well as the lateral, or that which arises from the draught is inversely as the diameter of the axis to that of the wheel; so that the amount of friction caused by a five foot wheel, is just half that of a wheel two feet six inches diameter, the axles in each case being the same,—that the perpendicular friction is proportional to the superincumbent weight on the axle,

and consequently the lateral friction is proportional to the same.

In the theorems which follow, the diameter of the carriage wheels will be taken at 12 times that of the axle; and, as it appears by actual experiment, that 1 lb. suspended on the circumference of the wheel of a carriage, will produce a minimum of motion of the carriage, when the superincumbent weight on the axle is 124 lbs.—the friction taken in the theorms will be 1 in 124, or $\frac{1}{124}$th of the weight. The numerator of the fraction being rejected, 124 will represent what will be termed the prime module of friction, and the module of friction on any given weight, will consequently be that weight divided by 124. It will be scarcely necessary to observe, that the module of friction so determined, being multiplied by the space passed over by the carriage, the product is the quantity of friction in such space; for the module so taken, is a representative of so much actual weight, since such actual weight is requisite to produced a minimum of motion or a minimum of friction.

Were this a merely elementary treatise, another course would have been adopted, but it is desirable to render the subject of friction intelligible to the simple mechanic; and it may be proper here to observe, that whatever error there may be in the experiment above made, it will be compensated for by Rule B, given afterwards. Such

rule will give the friction on the axle, that on the rail (if any), as well as the lateral friction on the edge of the rail, and the friction being so determined, theory and practice necessarily agree.

The mathematician who may examine the theorems which follow, may consider the prime module of friction as the unknown term, still to be determined, or he may take it as a mere approximation to it, as may be most convenient to his par ticular genius.

The next matter for consideration, is the adhesion of the driving wheels of the engine to the rails; and as this has been very significantly termed the bite, that term, for brevity, will be adopted herein-afterwards. It differs in principle from friction in this respect, that when it ceases, friction commences; nevertheless, mechanical writers have been accustomed to confound the two cases under the same expression.

If we suppose the surface of the rails, and circumference of the wheels, of the same material as the axles and bearings, and of the same degree of smoothness, as in the experiment whereby the friction was determined at $\frac{1}{124}$, and that the friction, when neither are oiled, is twice that which would arise when both are oiled, or 1 in 62, and when the wheels are to the axles as 12 to 1, then the bite on the rails would be $\frac{62}{12} = 5\frac{1}{6}$,—that is, if the pressure on the rails be $5\frac{1}{6}$ lbs., the bite, according to such theory, would be 1 lb.

By actual experiment, the wheels being of cast-iron turned, and the rail of wrought-iron rendered in some degree smooth, the superincumbent weight is to the bite as about 6 to 1, and which will be taken as the module of the bite; but the actual bite in practice will differ therefrom according to the construction of the engine. If its power be directly connected with only two of its four wheels, and supposing the centre of gravity of the engine to fall midway between the driving and the hind wheels, the bite would be one-twelfth the entire weight of the engine; but if the power be directly connected with the four wheels of the engine, so that they all become driving wheels, the bite would be one-sixth of the engine; but conceiving the former practice the more usual, the bite will be taken in the theorems which follow at one-twelfth the weight of the engine.

Whatever error or difference may arise in either case, will be corrected by Rule A. It may be proper to observe, that the module of friction, multiplied by the velocity of the train, is equivalent to the power requisite to raise a weight (equal to that expressed by the module) perpendicularly, the same height that the train is moved horizontally; so that the steam power requisite to move a train, at any velocity, on a level railway, is the complicate of the module and velocity. For example, if the weight of a train be 67,200 lbs., the module is $\frac{67200}{124} = 542$ lbs. and if the velocity be

1461 ft. per minute, the requisite power is 542 $\times 1461 = 791862$, which divided by 33,000 the power of one horse, gives the number of horse power requisite to propel a train at the above velocity, or nearly 24 horses.

It remains to be said, that as it is customary to estimate horse power in lbs. and the distance or height raised in feet, all weights and distances in the following theorems, will be taken in or reduced to the same standard, and that 33,000 lbs. raised one foot per minute $=$ 33,000 will be taken as a general expression for 1 horse power per minute.

Before proceeding with the theorems it may be well to observe, that if f be the prime module of friction (124), and 12 the ratio of the weight of the engine to the bite, the greatest weight of train, moveable by the engine, so that the driving wheels do not slip on the rails, is $\frac{f}{12} - 1 = \frac{124}{12} - 1 = 9\frac{1}{3}$. That is, the weight of train to that of the engine must not exceed $9\frac{1}{3}$ to 1, when the driving wheels only are moved directly by its power; and when the four wheels are so moved, the proportion must not exceed $\frac{124}{6} - 1 = 19\frac{2}{3}$ to 1. On ascending any gradient, the power is expended partly in friction, and in part in raising the engine and train up the gradient, so that the following theorems are constructed accordingly, and afterwards mathematically demonstrated.

By the term used in the theorems "the highest practical gradient" is to be understood, the highest gradient an engine can ascend with a given weight

of train, both in regard to its power, and so that its driving wheels do not slip on the rails, or lose their bite; but it is evident, that in practice, the gradient must be somewhat less than that assigned by the theory, or the weight of train somewhat less than the proportions taken. But the nearer the weight of train approaches to that taken, the greater will be the beneficial mechanical effect.

In the following theorems,

Let. $E =$ the weight of the locomotive engine in lbs.

$T =$ " " train.

$W =$ " " engine and train.

$P =$ the power per minute expressed in lbs. multiplied feet.

$f =$ the prime module of friction.

$F =$ module of friction.

$B =$ the bite on the rails in lbs.

$G =$ the highest practicable gradient.

$V =$ the velocity in feet per minute in ascending that gradient.

$v =$ " " on a level railway.

$w =$ the weight in lbs., or the tendency (when at rest) of the engine and train to descend any gradient or inclined plane by the force of gravity, $=$ the power required from the engine to maintain itself and the train in equilibrio with the force of gravity, when at rest on any inclined plane or gradient, $= B$ the bite when on the highest practicable gradient.

Theorem 1. $\frac{E}{12} = B$, the bite on the rails, and also $= w$ in case of ascending the highest practicable gradient, when the bite is to the superincumbent weight as 1 to 12.

Theorem 2. $\frac{12\,W}{E} = G$, the highest practical gradient.

Theorem 3. $\frac{W}{f} = F$, the module of friction.

Theorem 4. $\frac{P}{F + B} = V$, the velocity in ascending the highest practical gradient, and $\frac{P}{F + w} =$ the velocity in ascending any other gradient.

Theorem 5. $\frac{P}{F} = v$, the velocity on a level railway.

Theorem 6. $\frac{W}{G} = w$, the force or tendency to descend any other than the highest gradient.

Theorem 7. $\frac{P}{33{,}000} = \frac{V \times \overline{F + w}}{33{,}000} =$ the number of horse power required on any gradient.

Theorem 8. $\frac{P}{33{,}000} = \frac{v \times F}{33{,}000} =$ the same on a level railway.

Theorem 9. $\frac{P}{33{,}000} =$ the number of horse power required on any inclined plane or level railway, when the velocities, &c. are known or given.

The following cases exemplify the foregoing theorems.

			lbs.	
CASE 1.	An engine of 24 horse power weight 6 tons	=	13440	= E
	Train, . . . „ 24 „	=	53760	= T
Given.	Engine and train, . „ 30 „	=	67200	= W
	Power of engine 24 horses × 33,000 lbs.	=	792000	= P
	Prime module of friction, (by experiment)	=	124	= f

Required, the highest practicable gradient, the velocity on ascending it, and on the level railway.

Theorem 1. $\frac{E}{12} = \frac{13440}{12} = 1120$ lbs. $=$ B, the bite, and $= w$ in this case.

Theorem 2. $\frac{12\,W}{E} = \frac{67200 \times 12}{13440} = 60 =$ G, the highest practicable gradient, being a rise of 1 in 60.

Theorem 3. $\frac{W}{f} = \frac{67200}{124} = 542 =$ F, the module of friction.

Theorem 4. $\frac{P}{F+B} = \frac{792000}{542+1120} = 476{\cdot}5 =$ V, the velocity in feet per minute up the gradient, or 5·4 miles an hour.

Theorem 5. $\frac{P}{F} = \frac{792000}{542} = 1461 = v$, the velocity on a level railway, or 16·6 miles an hour.

In this case B, the bite, is equal to w, the tendency to descend the plane by the force of gravity, consequently the driving wheels might or might not slip on the rails.

CASE 2.—Given the same as in case 1, except that the gradient there found to be 1 in 60, is here taken at 1 in 120 $=$ G.

Required the velocities, &c.,—

Theorem 1. $\frac{E}{12} = \frac{13440}{12} = 1120 = B$, the bite.

Theorem 3. $\frac{W}{f} = \frac{67200}{124} = 542 = F$, the module of friction.

Theorem 6. $\frac{W}{G} = \frac{67200}{120} = 560 = w$, the tendency to descend the gradient.

Theorem 4. $\frac{P}{w + F} = \frac{792000}{560 + 542} = 718{\cdot}7 = V$, the velocity per minute, or 8·17 miles an hour up the gradient.

Theorem 5. $\frac{P}{F} = \frac{792000}{542} = 1461 = v$, the velocity on a level railway, as in Case 1.

It is obvious in this case, B, the bite, exceeds w, the tendency to descend.

Case 3.—Given the velocity on a level railway 1461 ft. per minute; the gradient 1 in 120; the velocity thereon 718·7 ft. per minute; the weight of train and of engine as before.

Required, the number of horse power to produce these velocities,—

Theorem 7. $\frac{P}{33000} = \frac{V \times \overline{F + w}}{33000} = \frac{718{\cdot}7 \times \overline{542 + 560}}{33000}$ $= 24$ horse power on the gradient.

Theorem 8. $\frac{P}{33000} = \frac{v \times F}{33000} = \frac{1461 \times 542}{33000} = 24$ horse power on the level railway.

The exceptions to these theorems and cases being strictly borne out in practice, exclusive of what has been already mentioned, and will be afterwards accounted for, are, that the bite would be in some small degree increased or lessened on as-

cending a gradient, according to which end of the engine might be moving foremost, as thereby the centre of gravity of the engine would approach or recede from the driving wheels. Also that the carriage wheels, forming no part of the friction *on* the axle, about four-fifths of their weight should be deducted from the weight of the train to determine the friction F; and the same proportion of the weight of the axles should also be deducted, if they revolve with the wheels. The atmospheric resistance has not yet been accounted for; and it is evident that, both on the inclined plane and on the level railway, the effective power of the engine is taken at a maximum. If to these exceptions be added, that the lateral friction of the margin of the wheels against the rails, which scarcely admits of calculation, has not been allowed for, the theory given should be borne out by experiment, with all but mathematical accuracy; when the corrections as to the resistance of the air, the bite, and actual power of the locomotive, shall have been determined by the experimental rules subjoined.

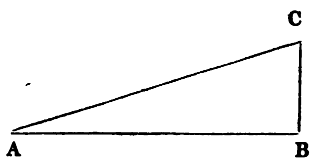
Proof of the Theorems.

Theorem 1. $\frac{E}{12}$ = the bite on the rails. This is deduced from experiment, as explained (p. 55); but if the bite be found by rule A to exceed one-sixth or one-twelfth the weight of the engine, in either of the two causes before described, it would be enabled to ascend a higher gradient than shown by theorem 2, its power and weight of train being

the same, but the velocity would be decreased on ascending the higher gradient. The decrease in velocity would not be in the same proportion the gradient is increased; for in both cases the friction would be a constant quantity, and the tendency of the train to descend variable. But on ascending a gradient, being less than the highest practicable, as established by theory,—as in no case would it be proper that an engine be made to ascend such gradient, for in such case the wheels might or might not slip,—the theorem is practically true.

As to the bite on a level railway, it need only be said, the weight of the train to that of the engine must be somewhat less than the proportions given at page 57.

Theorem 2. $\frac{12\,W}{E} = G$, the highest practicable gradient.



By Emerson's principles of mechanics (Prop. 31), "If a weight, W, be sustained on an inclined plane A C, by a power acting in direction parallel to that plane, then the power that sustains the weight is as the length A C to the height B C;" consequently, if we make $BC = 1$, and $AC = G$, G will denote the ratio of A C to B C; then if W be equal to the weight of the engine and train,

$\frac{W}{G}$ is equal to the tendency of the engine and train to descend the plane by the force of gravity, and consequently the power required to keep it in equilibrio. The power then, on ascending the highest practicable gradient, is equal to B, the bite; for were the bite less than the power, or less than the tendency of W to descend the plane, the train would descend. Then the bite being $\frac{E}{12}$, and the tendency to descend $\frac{W}{G}$, we have the following equation, $\frac{E}{12} = \frac{W}{G}$, and consequently $G = \frac{12\,W}{E}$ as per theorem.

Theorem 3. $\frac{W}{f} = F$, the module of friction. If f represents the prime module as found by experiment, and taken as a fractional part of 1, and if W represents any given weight $\frac{1 \times W}{f} = \frac{W}{f}$ is the module of friction proportional to that weight, and which is represented by F. Such module of friction is to be considered equal to so much actual weight as F expresses in lbs.

Theorem 4. $\frac{P}{F + B} = V$. The power required to ascend an inclined plane is in the complicate ratio of the friction and bite conjointly, when the highest practicable gradient is ascended; for the bite is equal to the tendency of the weight to descend, as proved above; that is, the power P is equal to $\overline{F + B} \times V$, and hence $V = \frac{P}{F + B}$.

Theorem 5. $\frac{P}{F} = v$. On a level railway the power

P is in the compound ratio of the friction and velocity, therefore $v = \frac{P}{F}$.

Theorem 6. $\frac{W}{G} = w$. On ascending any inclined plane, on which the tendency of the weight to descend is less than B the bite. "The relative force of the body (Emerson, Prop. 31, Corol. 2), is as the height directly and the length reciprocally, or it is as the sine of the plane's elevation. Therefore $\frac{W}{G} = w$, the weight or load on the engine, when at rest on any incline, = the tendency of the engine and train, to descend by the force of gravity, = the power required to maintain itself and train in equilibrio with the force of gravity on any inclined plane.

Theorems 7 and 8, being deductions from the foregoing, require no proof, excepting that if 33,000 lbs. raised a given height in a given time be the standard of one-horse power, then the total weight or power in lbs. multiplied by feet is the number of horse power required to overcome that resistance in the same time.

Rule to determine the actual Bite.

(A.)

PLACE the locomotive on a level railway, and fix the driving wheels so that they cannot revolve. If the other wheels move independantly of the driving wheels, leave them at liberty, but if moved direct from the power of the engine, and dependantly on the driving wheels, fix them in like

manner as the driving wheels. Next attach to the engine a rope, to be passed over a pulley, to which is suspended a weight. Let the weight be such, as to produce in the locomotive a minimum of motion on the rails, and such weight is nearly the actual bite. If to the rope, between the engine and the pulley, a dynomometer be attached, the bite will be accurately ascertained from its index, as it will account for the friction of the rope in passing over the pulley. Then the weight of the engine having been ascertained, and in case both its driving and aft wheels be moved directly by its power, its weight being divided by the weight indicated by the dynomometer, will give the ratio of the bite to the superincumbent weight, applicable in all cases, wherein the parts in contact, viz., the wheels and rails, are of similar material, and the same degree of smoothness, as that used in the experiment.

If the aft wheels of the engine move independantly of the driving wheels, ascertain the superincumbent weight on the driving wheels by placing them on a weighing machine, whilst the aft wheels rest on the ground: then proceed, as in the previous case described, and the ratio of the bite will be known, by proceeding as described in the preceding case. When such experiments shall have been made, the result may be substituted for that taken in the theorems, at one-twelfth the weight of the engine, when theory and practice, will approximate as

nearly to each other, as can under ordinary circumstances be expected or desired.

Rule to determine the Actual Friction.

(B.)

THE diameter of the axis of the carriage to that of the wheels, and the weight of the loaded carriage being ascertained, and a dynomometer placed as above described, let such weight be applied to a rope passed over a pulley, as will produce in the carriage a minimum of motion, and the total weight of the loaded carriage, inclusive of its wheels and axles, being divided by the weight or force indicated by the dynomometer, is the ratio of the prime module of friction to 1, when the diameter of the wheels to that of the axle, is in the proportion used in determining such module.

From this experiment the friction on all axles, the workmanship and material being similar to that used in the experiment, may be determined, whatever may be their diameter to that of the wheels, the friction being inversely, as their diameter, to that of the axle.

The prime module of friction being thus found may be substituted for that taken in the theorems as 124.

The prime module of friction on the axle of the locomotive engine, may be determined in a similar manner, the connecting rods being disconnected from the crank axle.

Rule to determine the Beneficial Power of a Locomotive Engine.

(C.)

Let the engine be set to work on a level railway, the weight of the train being known, and the friction having been ascertained by Rule B, and when at full speed, note the time during which it has passed over a certain number of feet.

If the time noted be less than one minute, increase the space passed over, so as to represent the space in feet, that would be passed over in one minute. If the time noted be more than one minute, reduce the space to that which would be passed in one minute; then divide the weight of the train in lbs. by the prime module of friction as found by Rule B, and the quotient, multiplied by the feet passed over in one minute, gives the beneficial power in lbs., and which, being divided by 33,000, is the beneficial number of horse power of the locomotive engine.

It scarely need be observed, that in making such experiment, the quantity of steam admitted to the cylinders, must be such only as the boiler can continually maintain, but it may be proper to remark, that as the locomotive is moved by its own power, the greater the weight of the train in the experiment, so that it does not exceed the ratio above given, the greater the beneficial effect produced.

Rule to determine the actual power of a locomotive engine.

(D.)

The actual power of a locomotive engine exceeds the beneficial power, by the friction produced by its own gravity; consequently to determine it, suppose the weight of the train 30 tons, and the engine 6 tons, its wheels and axles being to each other in the same proportion as those of the train, the actual power is to the beneficial power as 36 to 30. If the wheels and axle of the engine differ in proportion from those of the train, the difference being made proportional to the weight of the engine must be added to or deducted from 36, as the ratio of the wheels to the axles of the engine may be greater or less than that of the wheels to the axles of the train; or the weight of the locomotive being known, and the prime module of friction ascertained by Rule B, proceed as in Rule C, substituting the weight of the engine for that of the train, to determine the actual friction, whilst the engine passes over a space equal to that which the train has passed in one minute. Such amount of friction being added to that of the train, the sum is the actual power of the locomotive expressed in lbs. multiplied by feet, which being divided by 33,000, is the number of actual horse power of the engine.

General Observations on Friction, as determined by Mr. Tredgold.

Mr. Tredgold on locomotion (p. 62) says—"We have considered friction to be one-eighth of the pressure, at the same time we are fully aware that it may be greatly reduced by good workmanship, &c." "A carriage selected and trimmed for the purpose of experiment, will never afford a fair measure of the average resistance on a railway." And "in preparing our model we have avoided that smoothness and accuracy, which could not be adhered to in machines for use."

However plausible such reasoning may seem, it will on consideration be found unsound. His ex- experiment (p. 49) shows the friction $\frac{1}{62}$ when the wheels are to the axles as 4 to ·55, and as $\frac{1}{113}$, when the diameter of the wheels is doubled. Hence the deduction (p. 55) that in the first case the friction is 0·1212, and in the last 0·12978, and the mean between them nearly $\frac{1}{8}$th, and that this will be about the friction in practical cases is an unparalelled absurdity; for in neither case is velocity taken into consideration, and therefore the measure of one-eighth cannot represent the amount of friction in a given time or space. Moreover it will be seen in the subsequent article, that he takes the bite at one-twelfth the weight; but one-eighth exceeds one-twelfth, consequently the friction according to such theory exceeds the bite, so that the

wheels of the locomotive should be constantly slipping on the rails!!—a theory which cannot be borne out in practice by any fair means.

As to accuracy of workmanship of a model for experiment, I disagree with Mr. Tredgold, unless he means to contend that, in machines for use, the inaccuracies of the model will be necessarily magnified as the machine is increased in weight and dimensions; in which case an axle, instead of being put into a turning lathe, might be left to its own discretion to round itself. I should rather have said, if it be desirable to ascertain the true measure or quantity of friction by model, the axle should be finished as accurately as possible; but by the mode described in Rule B, the friction will be determined more accurately than by a model.

It may be proper to observe, that by Mr. Tredgold's experiments as above referred to, the ratio of friction does not diminish precisely as the diameter of the wheel is increased, so that his experiment differs from the theory adopted, in establishing the preceding theorems. That difference, arises from his having neglected to deduct the weight of the wheels from the weight of the carriage, and in part by his having produced an uniformly accelerated velocity of the carriage, whereby he included, not only the friction but the vis inertia of the weight moved, so that the experiments were made on a mistaken principle.

General Observations on the Bite, as determined by Mr. Tredgold.

Mr. Tredgold's experiments (page 57), show it one sixth of the pressure, cast iron sliding on wrought iron, and both rather smooth with wear, but he says, in a case of this kind, we should estimate it at a less proportion, say not exceeding one-twelfth, for the surfaces become glazed and slippery by use.

If so, the bite on a glazed surface is to be considered less than the friction in usual workmanship, and when the rubbing parts are well lubricated, which is a gross inconsistency and undeserving a comment.

It is however, evident, on inspection of the above theorems, that at the higher velocities the bite is a secondary consideration, but at low speeds as is usual on railways attached to coal mines, when the weight of the train is great in proportion to that of the engine, the bite is an essential matter, and in fact, in case of a heavy train on the great railways, should the weight of the engine exceed the proportion previously described, the railway might be worn out in one-tenth of the time it ought to last. The best criterion to determine, if the weight of the train be in excess, is to observe if the railway or the driving wheels of the engine, become heated in any sensible degree.

Observations on the last two Articles.

THEY have been examined at greater length than their importance may have seemed to demand, a course taken in order to show a specimen of the mere plausibility usual amongst engineers of the present day, when contrasted with what appears sound reason. Such errors may have been detected and exposed before, but even if so, they ought not to have escaped the eye of the professor, who superintended the progress of Mr. Tredgold's work through the press, for to any ordinary mechanic exercising reason, each one should be evident on inspection.

This explanation is now given, in order that the reader's time may be spared, and this volume kept, in as small compass as the subject will admit of; but should the theories to be laid down be in opposition to those of any mechanic of repute, as they decidedly will be, the author will deem it an incumbent obligation, either to admit his error, or answer objections made through any periodical, should his attention be directed to the subject. Under other circumstances it is scarcely probable, he might be aware of the objection; for during the last 40 years, he has read but one mechanical work, and which is, in his judgment, the only one deserving the name, the few hours devoted to the

works, which have or will be quoted, excepted. The cause, is not the want of a desire to learn, what progress or otherwise has been made in mechanics of late years; but the absolute incorrectness, or utter negligence, in rendering the matter described, evident and succinct.

The description by Mr. Tredgold of the locomotive engine, and its performance at Hetton Colliery, is an example of the justice of such stricture; for although he gave particulars, descending to the minutiæ of the different grades of thickness of the plate-iron composing the chimney, he neglected to state the dimensions of the *primum mobilè*,—the boiler, (its diameter excepted). In consequence, the nearest approach that could be made towards determining its power, is, that it is somewhere between 7¼ and 20 horses, a difference too wide to be of value to the mechanic, who may be desirous to learn what force is required, to produce a given effect. Next, there is a considerable difference between the speed of the coal waggons as given, and their speed as calculated, from the diameter of the driving wheels of the engine, and their number of revolutions per minute. Had that account been entire, the preceding theorems would have been supererogatory.

It may be here noticed, in order to prepare the reader for what he may afterwards discover, that every received motion of force, or what is more usually termed power, will be overturned, that the

standard of a horse power, as fixed by Mr. Watt, is a delusion as applied to the power of steam, and partially so as respects animal force.

The mode to be adopted, in making evident the theory, which will be afterwards proposed as a substitute, is to proceed on that which has been so long in use, and then, by a retrograde process, to reduce it to a just measure; and although such method be circuitous, it will be of advantage; for the mechanic, who may be desirous to understand the subject in its true bearings, will be convinced, before being aware, that an inroad has been made upon the universally received theory, laid down by Dr. Hutton.

The mathematical deductions will be found, in every case, to accord with the result in practice, which may be a new process in their application to mechanics; nevertheless the author is aware of the liability to render an expression not strictly or mathematically correct; and should there be one of doubtful sense, he trusts the error will be exposed, in order that mathematical truth may be completely established. The order of the preceding theorems, their mutual bearing with each other, and their simplicity of construction as well as of their proof, is conceived to be in unison with the simple operation of nature's laws, and which are now being rendered mysterious by the most erudite ingenuity of mathematicians; so that, however incompetent to decide on the point, he is

nevertheless induced to submit for consideration,—whether there be not adopted at our educational establishments, a system of teaching—the suppositious or ornamental, to the prejudice of the essential and useful.

To proceed with an examination of the principles of the railway on its proper merits: that is to say, irrespective of what may have been said or done by others, we will first ascertain the resistance of the atmosphere to the progress of the train. The table published by Mr. Tredgold (p. 278), in his essay on the strength of metal, and taken from the experiments of Mr. Rous, Dr. Lind, and Colonel Beaufoy, is correct, as has been proved above. By that table, when the velocity of the wind is 102·3 miles an hour, the force on a superficial foot is 51·426lbs.,—at 56·5 miles an hour it is 14·638lbs.,—at 13·6 it is 0·915 lbs.,—and 6·8 it is 0·229 lbs.

We will take as a standard, in estimating the resistance to a train, that at 102·3 miles the resistance is 51·435 lbs. on each superficial foot, as by so doing it will reduce the number of decimal places, in case of forming a general table.

By what has been said above, on the force and resistance of fluids, it follows, that 51·435 may be termed the module of force obtained by experiment; and that such force, multiplied by half the space passed in feet per minute, will give the whole resistance per minute on each superficial foot of the train.

We will now suppose a surface of engine and train equal to a plane of 20 superficial feet, opposed to the atmospheric resistance, and at

15 miles an hour, the resistance would be equal to the power of about		.	0·44250	horses.
30	ditto	ditto	3·54055	„
60	ditto	ditto	28·32445	„
120	ditto	ditto	226·59560	„

It may be here noted, that the resistance, which at 120 miles an hour is found equal to the power of 226 horses, would by Hutton's rule be only about 1⅓ horse, the difference being as 176 to 1.

An inspection of the foregoing is a sufficient exposition of the cause whereby, at higher velocities, mechanical effect has decreased in the ratio found by experience; but as there will be no difficulty in reducing the resistance to a twentieth part by the formation of the tender, if it preceds the engine, or by the formation of the engine, if it advance foremost, the chief obstacle to the higher speeds may be considered as removed, when the speed does not exceed 60 miles an hour.

This being established, I proceed to say, that leaving out of consideration the resistance of the atmosphere, and whatever may be the ratio of friction, the transit of goods and passengers on a level railway may be effected at a less expense per ton per mile at high speeds, than at such as are less.

In saying so I admit a limit; but it will sufficiently prove the assertion to show, that the expense of conveyance at 60 miles an hour, is less than at 30 miles or 15.

For example, an engine of 12 horse power will take a certain weight of goods at 15 miles an hour;—an engine of 24 horse power will take the same weight at 30 miles per hour;—and an engine of 48 horse will take the same weight at 60 miles an hour; for when the speed is double, so is the friction; and as the whole power of the engine is expended in overcoming the friction, the power required is as the velocity.

The consumption of fuel in a given time we will take as the nominal power of the engine; but if the velocity be doubled, a given distance is passed over in half the time; if quadrupled, in a fourth of the time; therefore the consumption of fuel, in conveying a given weight of goods a given distance as is the distance, whether the speed be more or less; and so the cost of the fuel is same, whether the velocity be 15, 30, or 60 miles an hour.

The wages of engineers and attendants on the train, are inversely as the velocity; for the expense is as the time, and at 60 miles an hour, the time would be one-fourth of that employed if the speed were 15 miles an hour; consequently the same attendants would perform four times the number of journeys in an equal time, and take four times the weight of goods; so that in the comparison be-

tween 15 and 60 miles an hour, the wages would be in favour of the latter as 4 to 1, or the wages, which at 15 miles an hour, amounts to £400, would at 60 miles an hour be reduced to £100.

The first cost of engine power might, to the superficial observer, seem a drawback on increased speed, as the outlay on an engine of four-fold power would be nearly quadrupled; but the engine of larger power will perform four times the number of journeys in an equal time, and so will take four times the weight of goods in any given time; consequently the number of locomotive engines required, when the speed is 60 miles an hour, is only one-fourth the number requisite when the speed is 15 miles an hour; therefore the expense of engine power is the same, whatever be the speed on a level railway.

As to the power requisite to raise any weight a given height, it is the same whatever be the velocity; for if the velocity with which the weight is raised be doubled, the power for the time being will be double also; but the time of raising it will be diminished inversely as the velocity is increased, wherefore the whole power in both cases is the same, whatever be the velocity of the weight.*

Whence, in ascending any inclined plane or gradient, whatever be the angle of the horizon,—as the power requisite to move a train on a level railway, or to raise any weight perpendicular to the horizon is the same, whatever be the velocity

with which the weight is raised,—and furthermore, as every inclined plane or gradient makes an angle with the horizon between 0 degrees and a quadrant of 90 degrees,—the whole power requisite to ascend any inclined plane or gradient, with a given weight, is the same, whatever be the velocity on ascending it.

We are now prepared to revert to the subject under consideration in further detail, which is the expenditure on engine power, fuel, &c.

The first cost of an engine has been supposed proportional to the power; but the friction of a large engine is proportionally less as the power is increased; the power and strength of the parts being generally as the square or cube of the diameter, and the friction as the diameter,—hence neither weight nor workmanship are as the power, so that the first cost of a large engine is considerably less per horse power than of a small engine.

Next, as the friction in a large engine is proportionally less than in a small one, so the number of square inches of the cylinder to each horse power is also less; and the consumption of fuel in traversing equal distances, as well as of oil and tallow, is in favour of the larger engines; for the consumption of fuel is proportional to the steam generated, and the steam generated is proportional to the actual power.

It is then established beyond the reach of contradiction, that the first cost of engine power, and

the daily cost of fuel and wages in attendance on the train, are all in favour of high speeds on railways; and which circumstance will, as time advances, and as the principles of locomotion on railways shall be better understood than at the moment these lines are being written, extend the system of railway conveyance throughout the entire surface of the habitable globe.

THE CONSTRUCTION OF RAILWAYS.

In entering upon the subject it is proper to observe, that what follows may not in all cases be strictly applicable as to minutiæ, to the requirements of Great Britain. The strength of material is designed to carry such weights as pass over the railway between Manchester and Liverpool, a sample of which rails are laid down in front of the custom house at Sydney. In what follows, it has been assumed, such rails are not adequate to bear the load of one carriage, or an engine weighing more than six or eight tons. The railway to be described afterwards, is proposed as a substitute, and is calculated to bear an equal burthen; nevertheless, to ensure as far as human sagacity can foresee the future, and to prevent such frequent accidents

as are being detailed in newspapers, it has been deemed proper to compute the requisite strength as compared with the railway mentioned, so that as to vertical pressure it is as 6 to 1,—as to lateral pressure as 9 to 1,—as to weight at about 5 to 1, and as to general stability, the comparison may be considered infinite.

The railway to be proposed will be of wood, but its duration has not been ascertained; although after an exposure to wet and dry for 35 years, it has been found as sound in the climate of Australia as on the first day. The duration of timber is perhaps the only matter in this book, on which an opinion will be hazarded; and that the author's opinion might not stand unsupported, a few months before quitting Sydney, he addressed a letter to his Excellency the Governor (supposing that, amongst the government architects and surveyors, an opinion might with little trouble be collected, as to the duration of timber for such purpose, and three others, matters intimately connected with the subject). It must have been obvious to his Excellency, the answers were not designed for the author's personal profit, because the answers were requested to be sent to the home government. That application was unsuccessful, and the following answer in his Excellency's handwriting is now transcribed.

John Curr,

"Enclosing a manuscript work on the Principles of Locomotion, as applied to steam."

"I regret that it will not be in my power to assist Mr. Curr in the way he suggests. C. F. Dec. 19."

The author then will be exonerated from culpability, in setting forth his own opinion, as to the probable duration of timber, when a better might have been produced.

For the purpose of railways, wood is decidedly preferable to iron, as regards its non-expansion lengthways by variation of temperature; and the iron-bark tree of New South Wales, from its hardness and great specific gravity, is probaby not to be surpassed by the timber of any part of the world. Its specific gravity, as compared with oak timber, is about the proportion of 14 to 10. Iron-bark is abundant everywhere in the middle district, being that in which Sydney is situate.

It may be proper to observe that, although the expense of conveyance at high speeds, is considerably less than at a lower speed; increased speed seems to imply an increased risk of life and property, unless the railway be constructed in a manner more accurate, than is practicable at any reasonable cost, if made of iron.

If then we contemplate the saving both of time and expense by an increase of speed, it may be inferred, there will ever be a desire, on the part of one company to outstrip another, provided it can be done with personal safety to the passengers; and hence arises the necessity of rendering the consideration of personal safety predominant over every other.

Exclusive then, of the method to be proposed

in passing curves, and of a proposition to reduce (in case of emergency) the speed of the train down any decline; or when from any cause, as the prospect of coming in contact with another train, which will be examined in its proper place, it seems that safety depends, rather on the perfect accuracy of the railway, its strength, solidity, weight, and the connection of one part with another, than on the absolute speed of the train. By such proper and accurate construction only, can the wheels be kept in the proper track; and in fine, if we except actual collision, there seems little more danger from an increased speed, than at such speeds as appear already to have been attained, although the force with which opposite trains meet is as the square of their velocity, if their weight and velocity be equal. Such at least is true according to Emerson, quoted above, and of which there is no reason to doubt.

Extreme accuracy, then, accompanied with what has been named above, may be taken as the measure of safety, provided the rails be connected together so that the disturbance of one, cannot by any force be effected, without also removing several. In the plan afterwards given and described, such accuracy has been aimed at, and there seems to the author no good reason to doubt of its being effective.

To describe the proposed wooden railway in detail, it may be first observed, that experience alone can determine if it be necessary to protect it

by plate iron, for the reason given above, and also on account of hard wood being more susceptible of polish than soft. The experiment however deserves to be made, as springs to the carriages might be dispensed with, and perhaps the buffer, the apparent utility of the latter being in passing a curve, or previous to the starting of the train.

In sawing the timber, it would be desirable that the length of the rails be such, as would make a given number of sleepers; for supposing the scantling of both to be the same, that of the best grain would be selected for rails, and the rest cut into sleepers without waste. If the depth of the rail be twice its width, it will not only be stronger as to the perpendicular pressure than square timber, but in case of a curve the bending would be more uniform, as well as more easily accomplished. This might in a great measure be done, as soon as the scantling is sawn, as, by supporting the two ends, its gravity would give it a regular sweep; and with little trouble or expense, different sweeps could be formed, whilst the timber is green to suit different radii, and such uniformity of curve would greatly add to general safety.

To give lateral strength, and to place and preserve the two lines of rail composing the single railway perfectly parallel, a piece of scantling, 4½ inches square, is supposed to run on each side of every rail, the entire length of the railway, so that a rail would be immoveable from any cause, unless

the foundation should give way. The scantling last described should be so laid, that the joints of the internal pieces cross those of the external, and *vice versâ;* so that, when the whole shall have been connected together, as will be afterwards described, the railway may be considered immoveable by any reasonable force.

If throughout the entire length of the line, and underneath each rail, the slabs of the timber after being charred, be laid longitudinally for the sleepers to rest upon, the railway might be fixed with almost mathematical accuracy, by wooden packs or gluts between the slab and sleepers, of which a quantity of all thicknesses would be provided by the sawyers; and if a very weighty roller of a particular construction were slowly passed over the slabs a few times previous to laying the rails, no after settlement could take place after being securely treenailed, unless the foundations were washed away.

The roller most fitted for the purpose would be composed of three rollers fixed in one frame, and following each other, so that the slabs at their joints would not be more pressed than at their centre.

The sleepers are supposed to be three feet from centre to centre, and a railway so fixed, it would be sufficient to examine once in six months, which by a proper machine for the purpose, could be most effectually done, at a speed of several miles an

hour, and the smallest defect or irregularity detected. The difference of expense in surveillance, as compared with that on the Eastern Counties railway, as taken from the evidence to be afterwards noticed, would, in about seven years, repay the cost of the superstructure of the wooden railway.

It would be desirable if possible to dispense with an iron facing, both on account of the greater ease to the passengers,—the convenience and increased safety which might, by proper arrangement, arise from the diminution of noise—and more especially, from what may be conceived the greatest obstacle to the perfect safety of an iron railway, and which arises from the continuous jar and chatter of such great weights moving over it. To estimate the strength of an iron rail intended to support a continued weight is simple, but there does not seem any practicable mode of estimating the effect which will sooner or later ensue from the cause just described, and moreover, the same cause must inevitably tend to destroy the wheels and axles of the waggons and engines.

It will then be evident to every mechanic, who considers the respective qualities of timber and iron, that the former is decidedly preferable, in so far as regards the matter under consideration. If however it be indispensable, a plate of angular iron might be secured to the wooden rails, so that every objection raised, excepting that of the noise would be avoided, and which it would probably consi-

derably diminish in comparison with the present system. To ensure safety, it is not only requisite that the rails be parallel, but that the width of railway exceed that of the external flange of the wheels, in the smallest degree practicable. This, it appears, might be done more perfectly, in case of a wooden than an iron railway; and, moreover, as the internal width of the railway may increase by wear, a mode of adjustment might be promptly effected, without materially interrupting the traffic. Another considerable advantage, not only as to expense, but personal security, would arise from the expedition with which the rails might be elevated, by means of the packs above mentioned, in case of passing over a soft surface or sandy foundation. If the bite of iron upon wood be found sufficient, as it probably would be, excepting in the case of a heavy and slow train, instead of using angle iron, a batten about four inches broad, and one inch thick, might be substituted for the angle iron. For such purpose, the box tree of New Holland, a wood still heavier than iron-bark, and at present, used for the shafts of carriages, might be desirable.

The practicability of firmly securing it, is the only objection that presents itself; but as a flat pointed nail, $\frac{1}{8}$ inch broad, and $\frac{1}{16}$ inch thick, being filed smooth, and driven $\frac{1}{4}$ inch only into iron-bark, required $1\frac{3}{4}$ cwt. to extract it, when the weight was applied in direction of the length of the nail, there is no reason to doubt the practicability of

fixing the batten; more especially, as supposing the weight of train not to exceed the proportions given, the batten, either on a level railway or on any practicable gradient, would not be disturbed longitudinally, in case of its having no connection whatever with the railway.

In case a facing of iron be found necessary, it would be proper to make it of angle iron, the thickness of which, for ordinary traffic, being about ¼ of an inch, and the faces each 2¼ inches. The mode of fixture to the rails, so that it be not disturbed by contraction and expansion, or the nails or screws drawn or loosened, by which their heads might interfere with the flanges of the carriage wheels, is material.

It might be fixed with nails without heads as shown in Fig. 1. The two holes nearest the centre of the angle iron plates, being the size of the nails, and the others oblong, so that the nails being at right angles to each other, and the plates well panned and cramped at the time of being nailed; and a space left between each plate, proportional to the temperature of the atmosphere at the time of their being nailed, to allow for contraction and expansion, they would be as firm and secure as if fixed by screws or any other means. Besides the advantages of such mode it may be observed, that if screws were used at right angles, or nails, it would be impossible to extract either, after being worn and rusted, without tearing the wood to

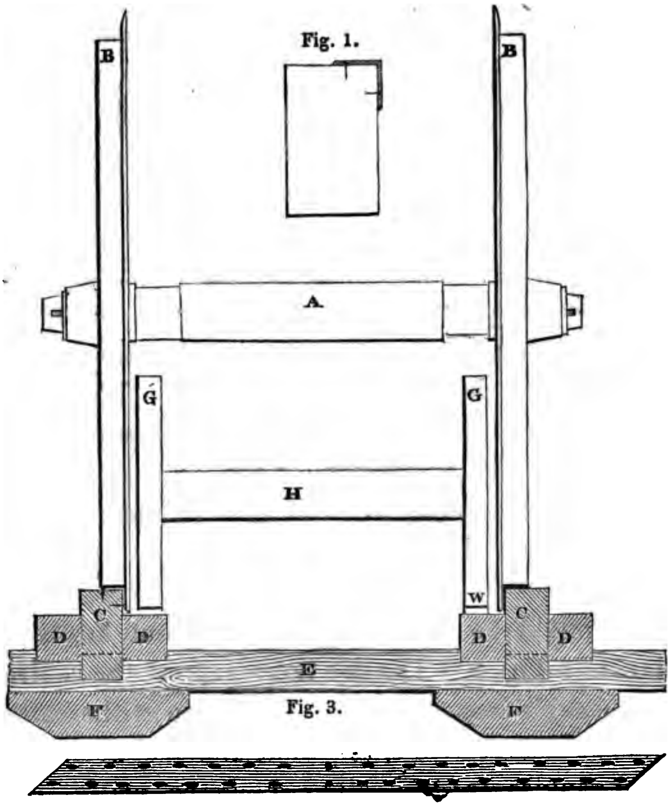


Fig. 1.

Fig. 3.

Fig. 2.

Explanation of Fig. 3.

A. The main crank axle of engine.
B. B. The driving wheels.
C. C. The rails 9 × $4\frac{1}{2}$ in.
D. D. The side pieces or lateral stays $4\frac{1}{2}$ in. square.
E. The sleeper, 9 in. × $4\frac{1}{2}$ in.
F. F. Slabs for sleepers to rest upon, continued throughout the line of railway.
G. G. Two extra wheels to be fixed on an extra axle H, to be supported by the main sides or runges of the engine. When the railway is straight, neither of the wheels, G G, would bear on the side pieces, DD, or revolve; but in case of a curve, a strip, W, is required on the external radius, which would be of such thickness, that when one of the weeels G, comes in contact with it, would raise one of the driving wheels, B, about a quarter of an inch clear of the angle iron on the rail, C. The engine in proceeding in the curve would be supported on two of its aft wheels, one of its driving wheels, and one of the wheels, G; and as the wheels, GG, move independantly of the engine, as to their central motion, its speed would be exactly proportional to the additional width of the

pieces; but by driving the nails without heads, either on the face or side of the plates, it would be removeable without damage to the wooden rail.

The joints of the plates should be angular, so that the wheels would pass over them without jar, if the iron be of uniform thickness; that is, the form of the plates, if reduced to a flat surface, would be as shown in Fig. 2.

Fig. 3 is a section of the proposed railway, and a description follows of the method of passing curves:—the only observation which need here be made is, that as in no case would it be prudent or proper, to make any sensible curve, except on a level part of the line, the bite of one driving wheel is ample, excepting at very low speeds.

Before proceeding further with the subject it may be advisable to give an idea of the probable expenditure on a railway as now proposed, in order to which the following preliminaries are given.

Sawing Hard Wood Scantling.—The customary measurement of sawyer's work in New South Wales is so exceptionable, that it is difficult to ascertain precisely, what would be the actual cost

railway as compared with the radius of the inner curve. Therefore it would have no tendency to cause the engine to quit the proper track, and the two flanges of the driving wheels would always be in position to keep it on the line the same as on passing a straight line. The place of the wheels, G G, if shown on a side elevation of the engine would be a little in advance of the main axle, and in passing the line in one direction one of the wheels, G, would be raised clear of the angle iron by W, and in the other direction the opposite wheel would be so raised.

These additional wheels and axle might be of service in case of breakage of the main axle.

The rails, C C, would be joined by a vertical mortice and tenon.

of sawing railway scantling. The sawyer is paid by what is called superficial measure when scantling exceeds four inches square, and which in fact signifies that he receives as much wages for sawing a piece of scantling 12 inches square, as if the same were actually sawn into 12 boards one inch thick. When scantling is four inches square or under, it is paid for by running measure, or according to its length without reference to its other dimensions, at the rate of five shillings per 100 running feet, and which price is exactly equal to the actual surface sawn, at five shillings per 100 superficial feet, in case of one piece being cut out of one tree. But if a tree 20 feet long, be cut into 16 pieces, four inches square, it requires 10 cuts 16 inches deep, and the wages by running measure amounts to 12*s.* 10*d.* If only 8 cuts be made of the same length and depth, the scantling will be 4 × 8, and will consequently be paid for by superficial measure, and the wages amount to £1 1*s.* 2*d.* Therefore, by customary measurement, 8 cuts earn £1 1*s.* 2*d.*, and 10 cuts only 12*s.* 10*d.*

If the cost of sawing railway scantling be taken at six shillings per 100 of actual surface sawn, the rails and sleepers being 9 × 4½, the side pieces 4½ inches square, the sawing of each yard in length of a single line of railway is 32 feet 3 inches, being nearly 2*s.* per yard, or £220 a mile.

Cubic feet of Scantling in one mile of Single Railway.

	in.	in.	ft.	no.	ft. in.	c. ft.		c. ft.
Rails . .	4½	× 9	× 3	× 2	= 1 8¼	5·625 per yard	=	9900 per mile.
Side pieces,	4½	× 4½	× 3	× 4	= 1 8¼			
Sleepers .	4½	× 9	× 8	× 1	= 2 3			
	in.	in.	ft.					
Slabs (supposed)	18	× 5	× 3	× 2		3·75 „	=	6600 „

Weight of single Wooden Railway one mile in Length.

Sleepers, rails, &c.	9900 cubic feet	at 28½	per ton	is 348	tons per mile.
Slabs . . .	6000	„	„	„ 232	„
Total weight per mile of a single line of railway,				580	

Freight from Sydney to London.—Bones packed in casks are charged £1 per ton, and when loose only ten shillings, as in the latter case they are placed in the interstices left between other packages; in the estimate which follows the freight of hard wood scantling will be taken at £1 per ton, although from the great weight of hard wood, the ship owner will be much better remunerated than by the article of bones, &c.

Angle Iron.—The weight per yard will be about 12 lbs., taking the thickness at a quarter inch, and each face 2¼ inch broad, and the single line of railway would require nearly 19 tons per mile. Such dimensions, however, may be increased, according to the weights intended to pass over them.

Labour in preparing Rails, &c. and laying them down.—If this be done by system (as is supposed) three-fourths of the carpenters' work may be done by an ordinary labourer, as accurately and expeditiously as by a carpenter. If the railway be commenced at one end, and the rails distributed on the line as it proceeds, the expense inclusive of tree-nails (if of wood), and fixing the angle iron, ought not to exceed 2*s*. 6*d*. per yard, or £220 a mile. This estimate may be considered too small—it is certainly doubtful, but after due consideration it appears ample.

Estimate of the cost per mile of a single line of Railway.

Sawing	£220	0	0
Freight on rails, sleepers, and side pieces, 340 tons . .	348	0	0
„ Slabs . . .	232	0	0
Angle iron, 19 tons at £14, (including conveyance) . . .	266	0	0
Labour preparing and laying .	220	0	0
	£1286	0	0

This estimate is exclusive of the carriage of timber in New South Wales to the place of shipment, also of the same from the place where landed in England, to the railway whereon it may be used,

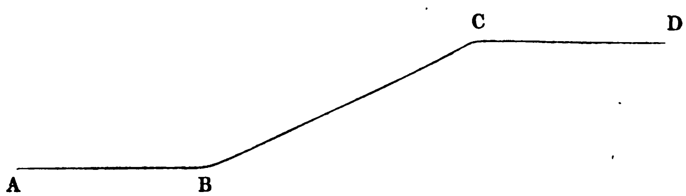
also of filling the earth within the rails, and of tunnelling, cutting, embanking, levelling, surveying, piling, draining, bridges, aqueducts, pass byes, turn tables, &c., so that in fact the estimate of £1,286 per mile may be put in comparison with the cost of the iron rails, chairs, sleepers, keys and bolts, and labour fixing a single line of railway, as at present being constructed, and being one mile in length.

At the conclusion of the preceding theorems it was observed, that the engine had been taken at a maximum both on a level railway and on an inclined plane, and as that may seem an impossible case, it may be here observed that on any considerable gradient an additional or stationary engine may be required to give the desired velocity, but in case of limited traffic the following method may be adopted to avoid such expense.

Let the engine be stopped near the foot of such inclined plane,—let the driving wheels be removed, and a pair substituted, being of such diameter as will enable it to ascend. Let us suppose the diameter of the substitutes one-half of the wheels used on the level railway, and the nominal power of the engine will be doubled, but its velocity will be reduced in the same degree. Therefore, the same engine, by the addition of an extra pair of wheels, will perform the work of two, excepting that the speed up the gradient will be proportionally reduced ;—the only apparent objection to which is that a small portion of time would be occupied in

changing the wheels, but which, by a properly contrived fixed screw-jack, would be done in three or four minutes, a delay which, in many cases, would be very inconsiderable in comparison with its advantages.

When any gradient or incline be of such rise, that it cannot be ascended at the desired speed by a locomotive engine, it would seem necessary to employ either additional power or a stationary engine; to supersede which on railways, subject to limited traffic, the following mode is proposed, and whereby any rise may be ascended by a locomotive engine, at nearly one-half the speed as with the same train on the level railway.



Explanation.—Let the lines A B and C D, represent the level railway, and B C the inclined plane to be ascended, and it may be ascended by a locomotive of ordinary power, with a train of the ordinary weight, as follows :—

Near the summit at C, and in a direction towards D, let there be erected a rope barrel, furnished with a break, and two ropes exceeding the length of the plane. Let the engine, with the train

at B, about to ascend by the power of the locomotive, be attached to one rope, whilst a number of drugs loaded with stone or other material, are attached to the other rope at C, so that whilst the drugs descend the plane, the train would ascend. The weight of the drugs, and the weight of the train, would be in equilibrio, so that when the locomotive is set to work up the plane, the only resistance to be overcome is the friction,—the friction would be twice that on the level railway, besides the extra friction of the rope and barrel; so that the speed in ascending the inclined plane would be nearly half that on the level.

There may be situations wherein stone or coal might be employed as the counterbalance, and sent to market, in which case it need not be again raised to C; or in case of a train being about to descend, it would have the desired effect, at the same time it would prevent the overspeed of the descending train down any considerable decline. In general, if in the aggregate, a greater weight passes down the plane than up, the drugs would be always at the top of the plane, when there required; and also at the bottom under the same circumstances, for should they be at the summit, when their services are there required, to prevent the too rapid descent of the downward train, they would be lowered by the break and their own gravity.

In other cases a fixed steam-engine of six horse power, applied to turn the rope barrel to

raise the drugs would, in most situations, be sufficient for that purpose, their velocity being regulated according to the nominal power.

The same principle, by a slight variation in the mode, might be adopted in case of ascending a hill on one side, and descending it on the other; thereby saving the labour of tunnelling or cutting it down, by which means a railway may become profitable, and the expenditure made proportionate to the traffic.

Hitherto the inclined plane has been made available, to the extent only of raising the empty waggons by the descent of those which are loaded, so that the invention described, however simple in principle, is probably new.

It is now desirable to consider what objections can be fairly raised against the railways, as at present in use in England; the first of which is, that both the strength and the mode of fixture seem inadequate to sustain the weights which pass over them, which is proved as follows:—According to the evidence on an inquest held some months since, in consequence of an accident on the Eastern Counties Railway, near Thetford; it appeared the engineer, for some cause not stated, very suddenly applied the break, when the train was moving down an incline at sixty or more miles an hour, so that the engine determined to *play the mountebank*, and made two successive leaps of some feet,—the fore part rising most, and, at the same time, the fire was dispersed in all directions.

Such is the effect which would be produced by too suddenly checking the speed of the fore part of the train or of the engine, so that the after part would abut against that preceding it, which also striking the engine, would, in case of the force of the blow being below the centre of motion of the engine, produce the effect described. No such explanation was given on the inquest, nor was any other hypothesis offered satisfactorily to account for the accident, therefore the reader will take that now given at its worth. On the same inquest it was given in evidence, that a superintendant and several labourers are employed in each mile and half of railway, to inspect each rail and the keys twice every day, and to adjust them when necessary; so that as to expense in this respect, it seems about £175 per mile per annum, and on such evidence it was said at page 87, that in case of the proposed wooden railway, its expense would be saved in a few years. A similar accident has since occurred on the Great Western, and apparently from the same cause.

The necessity of such constant surveillance, renders necessary a consideration of the cause, and it would appear first:—that if the keys be of iron, the jar and chatter of such weights passing along the rail, might disturb them,—that the same effect might be produced by the contraction of the iron by change of temperature; and if the keys be of wood, the rail might be loosened by its contraction

from continued dry weather. But whether the key be of wood or iron, if it be driven to excess, it seems possible the cast-iron chair might be so nearly fractured, that a separation would take place from the chatter of the engine in passing over it, and thereby cause an accident.

Next, the rails being fixed in the chairs by keys, and the chair to the sleepers by nails, without any lateral stay but the earth; there is little connection to bind the railway together, as one sleeper might give way laterally, without materially disturbing the next,—an objection which does not apply to the wooden railway previously described.

The narrow guage railways appear too narrow as regards the transport of bulky goods or live cattle, and the broad guage may be too wide, unless a mode be devised to assure safety in passing a curve. Under the system in use, as each of the two driving wheels of the engine, as well as each wheel of the pair, when fixed on the axle, revolves in equal times, every pair of wheels becomes a portion of a cylinder or roller. As a cone cannot revolve in a straight line, without grinding or producing friction against the ground, so a cylinder cannot revolve on a curve in a railway, without a similar effect being produced. When the wheels of the engine or train, being fixtures on their axles, revolve in a curve, the tendency of one wheel is to grind on the rail in one direction, and of the other to grind in an opposite direction. The tendency

is also, to cause the flanges of the wheels to grind against the internal margin of the outer rail, or that most remote from the centre of the curve. The consequence is, a general inclination to climb upon the external rail, and if there be an inequality in any of the joints, there is a possibility of the engine quitting the line of railway. This might be increased, in case of the break being applied, with greater force, on the wheel moving in the exterior line of rail, than the interior, and if the margin of the wheel should happen to come in contact with an uneven joint, at the same time that the piston of the cylinder, most remote from the centre of the curve, be passing that point, at which from the opposite impulse, consequent on its alternate motion, the greatest tendency to form a serpentine line is produced, the risk of accident will be at a maximum. Whence it may be inferred, that although a train may have passed on any railway one thousand times without an accident, that such experience is not a criterion of absolute safety; for circumstances which may be considered as accidental may not, in that number of journeys, have occurred, so as to unite, at any one moment, every cause whence accident may arise.

There are two causes always conspiring towards the engine quitting the rails, or overturning, on passing any curve, which are, the dragging of the wheels as just described, and the centrifugal force. The latter on any usual curve, scarcely deserves

consideration, but trusting to recollection, I think the broader guage is, by Act of Parliament, allowed to traverse in a curve of less radius than the narrower, and if it be so, such enactment has been made on mistaken principles. It is obvious that, on passing any curve, the train is kept on the rails by the pressure of the flange of the wheels laterally against the margin of the rails, that the greater the width of the railway the greater is the pressure, so that the greater the width of the railway the greater should be the radius of its curve; and if the external margin of the wheels be as usually drawn, viz., a portion of a plane, the risk of quitting the line is thereby increased, in comparison with the mode previously described of forming the margin; and in all such respects, the wooden railway, as well as the carriage wheels, have been so designed, as evidently will reduce the risk of accident to a minimum.

A short length of wrought iron railway, similar to that on the line between Manchester and Liverpool, is laid at Sydney in front of the custom house, on which the following remarks are made:—The upper surface of the rail is elliptical, and the external edges are portions of a circle, so that, as the external circumference of the wheels is cylindrical, they bear only on a particle or point, when theoretically considered. But viewing the case practically, we will suppose that point extended, so that it becomes a line, the length of which is the twen-

tieth part of an inch. The upper surface of the rail has probably been so formed, chiefly to increase the bite, but if so it is a mistake in principle, for the bite is not as the surface pressed, but as the weight which impresses it. This rule holds good, unless the surfaces be so reduced, that the super-incumbent weight, causes an abrasion of the parts in contact. Such is probably the case where the upper surface of the rail is elliptic, and if so, the crust of the iron soon disappears, the rails become bright, and the intention of the engineer is frustrated, so far as the bite may have induced his decisions in such form of rail.

The rails will be worn out in two-thirds the time they would endure if the upper surface were flat; and the same remark equally applies to the carriage wheels,—and on the whole; had the forge master planned the one, and the iron founder the other, a more successful effort to increase the trade of both, could not have been devised.

The same objection applies equally to the form of the internal margin of the rails, as to their duration, but in an inversed degree, as to general safety. Besides accuracy and firmness in fixing the rails, general safety requires that the *play* between the external margins of the wheel and the rail be a minimum, in order to reduce as much as is practicable the tendency to climb; the form of the edges being cylindrical, the *play* will be increased in much less time than if it were a plane,

so, by such form of rail, the liability of accident is increased,—and moreover, when it becomes necessary to substitute a new rail, in place of such as may have been bent or broken, the method in which the iron rail is fixed, scarcely admits of such accuracy as to the joints (especially as the new rail will differ in dimension from that removed) as is desirable.

Such objections, if not entirely removed in the wooden railway suggested, have been considerably reduced.

If, in these remarks, the author may appear hypercritical, the answer is, a cause imperceptible to the eye is not so to the intellect; and however small in appearance, every cause produces its natural result. The causes of railway accidents have not always been satisfactorily established, wherefore such as may not be self evident have been brought forward, and others are left to judge of their validity. In further reference to safety, as it has throughout claimed the first consideration, I will repeat, that a rail having been long in use, subject to variation of temperature, and perhaps occasionally, from actual friction on the upper surface, in consequence of the weight of the train exceeding a just proportion to the weight of the engine, may, from the repeated jar, to which it has long been subject give way, without exhibiting the actual cause of fracture. The longer then a railway has been in use, the greater the liability to

fracture from an imperceptible cause, and, consequently, increased accidents may be expected to ensue as time advances. The last observation is, the rails should be of good iron, but those at Sydney are most ordinary material, and obviously so on mere external inspection. They are not of iron properly so called, but of iron laminated with earth, so that the strength, as well as specific gravity, are less than would be if the material were proper.

It has been said above, that it is desirable to reduce as much as may be, the atmospheric resistance; but it is not to be forgotten, that, on descending any inclined plane, it has been of service, in keeping within bounds the velocity of the train. The atmosphere, although an invisible agent, has been an assistant to the break, and as the too sudden application of the latter might induce consequences as fatal as might be those intended to be prevented, it deserves consideration, whether a sail made of sheet copper, or other fit material, might not be employed in descending a gradient, so as to prevent a too great speed, in ordinary cases, and also relieve the break from a portion of its duties in case of emergency. Such sail or sails being applied at or towards the after part of the train, is more desirable than if nearer the engine, and if the proper cause has been assigned in the cases of accidents mentioned above, as to the fore part of the engines having risen from the rails on the sudden application of the break, such remedy seems de-

sirable. As such sail or sails would always be at right angles with the line of direction of the train, as their dimensions would be inconsiderable, and as they would be raised almost in an instant, it is probable, on the whole, general safety would be increased.

In the conveyance of grain of all kinds, and of all such matter as could be conveniently packed within a cylinder, so as to revolve therewith without damage; the friction would be reduced about 80 per cent. A cylinder of two tons weight would contain about twelve tons of wheat, and its cost, including the rims which would be required to keep it in the track, about £60. The friction would be that on the rails, which, if clear, may be taken at nothing, and that arising from the draft or the lateral friction on the axle, which after the train is under way, is in theory nothing; therefore an allowance of 20 per cent. should cover the actual friction on the rails and the draft in ordinary cases, and would therefore cause a saving of 80 per cent. in the cost (as well as the expense of fuel, &c.) of engine power;—the expense also of the cylinder would be proportionally less than of the usual waggons. Such method however would not be desirable, excepting the matter to be conveyed were considerable in quantity. Small coal might be so conveyed, and if on a level, the expense ought to be less than by sea over equal distances.

The author conceives that in the preceding

pages, expenditure, duration, repairs, speed, and safety, have each received the consideration it deserves,—he believes that he has reduced to a proper standard, that which although previously attempted has proved a failure. Should he have erred on any point which he has attempted to demonstrate mathematically, nothing will add so much to his satisfaction, as the exposition of the error, that he may be humbled accordingly. What has been merely suggested will be pardoned, if in opposition to any established fact, for with such he is little conversant, in the seclusion wherein he has passed the last twelve years.

If the theorems above be sound, engineers will be absolved from the necessity of picking them up piecemeal for the time to come. Once true, and they will be ever true, therefore the best workmanship as to material and construction, furnished at the lowest price, is the bone for which it will be their interest to contend.

As to the probable duration of iron-bark, Sir George Gipps, or Sir Thomas L. Mitchell, Surveyor-general of New South Wales, both of whom are probably in England, might give information.

A specimen of iron-bark will be left with the publishers for the inspection of such as desire to see it.

Actual collision of trains moving in opposite directions is a subject scarcely deserving attention, but as there appears a vulgar notion amongst per-

sons who ought to know better, that if two trains meet, the shock is proportional to their joint velocity, or to twice the velocity of each train, it may be said the shock sustained by each train is proportional to its velocity, and the same is true as respects each person conveyed by it.

Considerable interest was excited in Sydney by a wooden railway invented by Mr. Prosser, and in consequence the following comments are subjoined.

The experimental Wooden Railway designed by Mr. Prosser.

An engine weighing six tons passed over it, and during two months he estimated the work equal to the traffic of 12 engines a day for seven years, when he found the surface of the wooden rails as perfect as on the first day, for the saw marks still remained. This is unquestionably the natural result, for there is no actual friction on the surface of the rails, unless a weight of train be attached to the engine exceeding the proportions above described, so that if the rails were perfectly free from dust and sand, the saw marks might remain seventy years, provided the timber should continue sound for that time. The work however was not equal to that of 12 engines per day, with their trains for seven years as the reader might suppose, but to about one year only.

As to the wear of the internal sides of the rails

being the only part on which there is friction or rubbing of the parts in contact (for the groove of the guide wheels being conical, friction must ensue), the account published in Sydney did not make any mention.

Mr. Prosser, however, for some reason not sufficiently obvious has assumed, that the bite of iron upon wood, is greater than of iron upon iron, in wet and in dry weather, which appears to have been an inference from the circumstance of the engine having carried *itself* up a rise of 1 in 9 at a speed of 24 miles an hour. But the length of the railway was only 174 yards and the rise in the middle, so that it may be supposed the rise in each direction might not exceed 30 or 40 yards, in which case an engine without a train would be taken up the inclined plane by the impetus acquired on the level part of the railway, had the wheels no bite whatever. Moreover, taking the bite of iron upon iron at one-sixth the pressure, and which has been proved after both wheels and rails have been rendered smooth, the gradient that could be ascended without slipping of the wheels would be 1 in 12, by the preceding theorems, if only two wheels of the engine were directly connected with its power, and 1 in 6 if the four wheels were so connected, and without its being set in motion so as to have acquired a momentum on the level railway.

In whichever way the engine was constructed, there does not appear grounds for concluding the

bite to be in favour of the wooden rails in any considerable degree, as is proved thus :—the speed was 2112 ft. per minute—the weight of the engine, 13440 lbs.,—and which would require the power of $\left(\frac{13440 \times 2112}{33000 \times 9} =\right) 95\frac{43}{75}$ horses, even supposing that weight to have been taken up by a stationary engine, by means of a rope, and without any friction ; therefore, if it may be assumed that the engine used by Mr. Prosser was of less power shan 95 horses, the direct inference is, that the engine was taken taken up, not in virtue of the bite but by the momentum acquired on the level rails, so that the account published seems delusory.

It is however probable the bite of iron upon wood would exceed that of the usual material when both are dry,—when wet, unless the wheels were perfectly cylindrical and smooth, it might be not only much less but very uncertain, and on the soft timber employed by Mr. Prosser (scotch fir) it is probable, that by the depression of the surface on which the wheels rest, the effect might in degree be assimilated to a cart passing a sandy road, and whereby any advantage of bite might be amply counteracted by the power sacrificed.

STEAM NAVIGATION.

STEAM NAVIGATION.

INTRODUCTION.

THE subject is of such order, that with proper arrangement the theory may be comprised in a nutshell; so that the mechanic, using the term in its proper sense, would be enabled to adapt in the best way, the power to the purpose intended. But such dogmatic mode of instruction would flow with an ill grace, from one not possessing an influential name; and whose only claim to consideration is, the quality of the matter to be submitted for public scrutiny.

Such adaptation of power, comprises an essential part of the mechanical art; but as in the former part of this volume, both theoretical and practical errors were necessarily discussed; so in reference to the present subject, it may be fit, to

say at the outset, that every inch of ground to be passed must be disputed, before the heavy clouds which overshadow the path be dispersed, and mechanical practice rest on a firm foundation.

Since the introduction of the invention into Great Britain, about 35 years since, it seems to have undergone two variations; the one respecting the principle of the engine, the other the mode of propulsion. The variation in the construction of the engine was made in the year 1814,—its chief object being to enable the engine to be set to work instanter, or immediately stopped, without the risk of disruption of its parts, as might happen to an engine with the ponderous fly-wheel. The screw propeller is the second variation, and will be noticed afterwards.

Of all kinds of fanaticism, none is so unpardonable as fanaticism mechanical; and the revival of the expansive principle, to the extent to which it has of late been carried in steam navigation, is a subject deserving to be particularly examined. Expansion has been the fashion, not for its worth, but because engineers, doubtful of their own judgment, usually follow others, as sheep follow their leader, or puppies the trail of a red herring; so whether for good or for ill, the expansive principle must prevail, until put down by argument incontrovertible.

The principle was known to Mr. Watt, but not acted on, and why?—because he found no good

effect. It was tried in the Defiance, 33 years since, without perceptible benefit, and therefore was discontinued. Mr. Hoseason (*Mech. Mag.* vol. 43, p. 293) says, that of the three government vessels fitted with the expansive gear, not one of them were using it,—so that whatever figures may show on the subject, the expansive principle is a fair subject of criticism.

It is in fact an enquiry deserving greater consideration, than at first sight might appear necessary; for the cost of the engine of the Great Britain steam ship and the weight are that of about 1500 nominal horse power, whilst its actual power does not exceed 300.

The engines of Arthur Woolf, according to published accounts, who expanded the steam from 40 or 60 lbs. pressure, produced an effect greater than the usual condensing engine; but it was infinitely short of that, calculated by figures, and expected from his experiments. Whence then the advantage of his engine,—simply because it was contrived, or at least most usually employed, for a particular purpose,—the raising water from mines; the resistance was intermittent, and the power also, because it was variable.

Thomas Wickstead, Esq., C.E., (*Mec. Mag.* vol. 41, p. 310) in a paper written at the request of the commissioners of enquiry into the state of large towns, states, that by an expansive engine made by Harvey and Co., the expense of raising water

was £100, when by those of Boulton and Watt it was £220. But such evidence goes for nothing, because his facts and inferences are at variance;—he says, the comparison is favourable to the engines on the old plan, evidently meaning those of Boulton and Watt, and the reason assigned is, "because they were good ones!"—marvellous!—were they good because the cost of working them was more than double—or why?

The expansion is good, provided the cylinder be in a proper jacket; but the effect by figures is so deceptive, we will see their result; leaving out of the estimate the friction, and supposing the vacuum perfect, and that no radiation of heat, or external condensation of the steam takes place in the cylinder.

Then, 1 cubic foot of steam of the pressure of 229⅛ lbs. per superficial inch, is equal to one horse power in the usual condensing engine. The effect of the same if expanded 33,000 times would be nearly 183 horses!!!

The case has been put in the extreme, more decidedly to show the folly of relying on figures, when the antagonist principle of external condensation is not accounted for;—but however extreme the case, were the expansion doubled, the power by figures would be increased; and if steam may be supposed infinitely expansible, so would its power be also infinite.

Table 7, in the Appendix to Dr. Ure's Chemi-

cal Dictionary, shows that the elastic force of steam, increases in a vastly greater ratio than the temperature; a theory so liable to lead the mechanic into error, that it will be proper to examine it:—and although in the investigation, mere reason will be opposed against universally admitted fact, it is undertaken fearless of the result,—and it will be shown that of all misnomers, the term thermometer, is the most flagrant abuse of the English language. By that table where the temperature is 40° the force is 0·250, or as 160 to 1, and when the temperature is 280°, the force is 101·9, being less than 3 to 1, so that engineers relying on the table may have carried out the expansive principle without detection of its inefficiency.

We will proceed to examine the principle and construction of the thermometer.

THE THERMOMETER.

FLUID matter, within certain limits, increases in volume on acquiring additional temperature, and mercury has been generally used in ascertaining it; but as by the same increase of heat, different fluids dilate in different degrees, we are thereby enabled geometrically to graduate a scale, so that it represents, not only apparent, but relative temperature. The first enquiry will therefore be, whether such scale has been established, and if not, to produce it.

The Chemical Dictionary, (Edition 4,) by Dr. Ure, will be principally quoted as evidence on a subject avowedly of the utmost importance to the chemical philosopher, as well as to the mechanic; as, however, in all other parts of this book, it was found essential to uproot a variety of errors, in order that the truth might appear in its own simple dress, so in this enquiry it will also be found, that Dr. Herschel, when he said, (Babbage, p. 7) "In chemistry we have long since drawn the vein," was a pretty accurate judge of the quantum of mathematical science enjoyed by its professors.

The chemical philosopher may look down with his usual contempt, on the rude notions of a mere

mechanic, as may be augured from the tone in which Dr. Ure occasionally treats such reasoning; and, although with little more knowledge of the science than a dray horse, yet possessed of sufficient judgment to understand the principles of an instrument, so simple in construction, it is with no alarm as to the consequence that the subject is undertaken.

Chemical philosophers are already in dispute, as to the relative degrees of temperature indicated by the thermometer, whether it be that of Fahrenheit or Celsius; Clement and Desormes (Chem. Dic., p. 270) have been lately searching after the absolute zero, and are convinced that it is at 266·66° below the zero of the centigrade scale, or — 448° Fahrenheit. Dulong and Petit have been led by their investigation to fix the absolute zero at infinity; therefore, zero is an open question amongst philosophers :—It is, moreover, the only open question, and, therefore, must be settled.

The two scales above-mentioned, differ with each other, both actually and proportionally, when taken in reference to the boiling point of water and blood heat, as it may be proper to establish ere the subject advances.

Blood heat, by Fahrenheit, is	98°,	and boiling water,	212°,	or as	1 to 2·16.
Do. Celsius,	„ 36·6	„	100°	„	1 to 2·73.

Therefore, it is evident, both scales cannot be correct; if the scale of Fahrenheit be true, as to the heat of blood, in comparison with that of boil-

ing water, the scale of Celsius should give it 46·22°, instead of 36·6°, and if the centigrade be correct, blood-heat, by Fahrenheit, should be indicated by 77·73°, instead of 98°.

To prove that neither scale is correct, nor founded on a principle entitled to such pretensions, would only needlessly lengthen this paper;—we will, therefore, at once proceed to the examination of terms, in order that the one on which the investigation principally bears, may be rendered distinct and unequivocal.

Dr. Ure (p. 789) has written of the thermometer, "that Mr. Crighton's is almost an exact measure of temperature, or of the relative apparent energies of heat." Are we then to understand that exact measure of temperature, and relative *apparent* energy are always in agreement, or does apparent energy always indicate exact relative temperature? It may be inferred from the above comparison as to the heat of blood and boiling water, that such is not the case; and as zero on both scales has been settled, without the establishment of any data, it may for anything that appears to the contrary, have been decided by ballot or a cast of the dice: wherefore, without further argument on the case, apparent temperature is not a measure of relative temperature, unless the scale representing it be established on just principles.

It appears (Chem. Dic., p. 789) that the scales of

Fahrenheit, and of Celsius or the centrigade, are now principally used in Europe, and we may infer, that they enjoy an equal reputation, although the *relative* energies of each widely differ; but the reader is to understand, that the scale about being proposed, is designed to represent relative as well as apparent temperature of steam, according to its force or pressure.

As we are not entering upon a tale of fiction for the reader's amusement, it is desirable he should be made acquainted (*in limine*) with the author's design; which is primarily to establish a thermometric scale, representing actual force, pressure, temperature, and expansion of steam;—and which may be proved by an examination of the same in cases of its actual expansion, as well as of its expansibility. The purport of the enquiry is to disabuse the advocates of the expansive principle, now being put into such general use in steam-ships, of its fallacy. Such being accomplished, he proposes also to show, that the scale to be produced, will make void the fiction of latent heat; and, as he expects, will outroot it from the chemical vocabulary: since it would not have entered the conception of man, had the instrument used been a measure of relative temperature. Trusting to memory, Sir Richard Phillips has either doubted or derided the theory, and justly so; for if the scale in use be true, the following is forced upon us as fact, viz.,

that in mixing so much water with so much steam, a *miraculous* consequence ensues.

As to specific heat, Dr. Ure (p. 265) observes, that " Petit and Dulong *justly* remark, that the attempts hitherto made, to discover some laws in the specific heat of bodies, have been entirely unsuccessful." The want of success Dr. Ure ascribes, to " inaccuracy of the measurement," when he should have said to the inaccuracy of the instrument.

Dr. Ure's Dictionary of Chemistry, being the only book on the subject to which I have reference, and as I conclude from his usual tone, both, in respect to the science generally, to his brothers in the same, as well as to the lesser man the mechanic, who takes occasion to advert to it, that he is held in repute, and his dictum of weight, in scientific disquisitions; more frequent reference will be made to his errors, than charity or brotherly love might seem to countenance; but as I know no more of him than his name, and that it is followed by MD., F.R.S., and as it is unknown to me whether he may still exist or otherwise, it is not to be conceived, that in exhibiting his errors; a personal offence is intended.

To proceed in the explanation of terms, the distinction between mechanical and chemical action is necessary to be understood. Dr. Ure (p. 264) says, that on mixing water with alcohol, chemical action happens. On mixing water with alcohol I

say, that since the specific gravity of the mixture is not a mean, that is, an arithmetical mean proportional between the respective specific gravity of each liquid, the volume of the mixture, is not the sum of the two volumes, previous to their being mixed. In plainer language a pint of water being mixed with a pint of alcohol, the volume of the two will not be exactly two pints. This is so evident as not to require proof. If two equal quantities of water and alcohol, were so carefully put into one vessel as not to be intermixed; the lighter liquid would remain at the upper portion of the vessel containing the whole; in which case, the specific gravity of the whole would be a mean, for the entire volume would be the sum of the two separate volumes; but on mechanically agitating the containing vessel, the volume would be reduced, and the specific gravity consequently increased. Wherefore, mechanical action is requisite in forming the mixture, and the mixture is formed without chemical action; because no change takes place in the constituent parts of either portion of the mixture;—and, therefore, on mixing together alcohol and water, the action is not chemical but mechanical. To render obvious to the vulgar or popular reader, borrowing such terms from the Chemical Dictionary; if a pint of large shot be mixed with a pint of very small shot, and sufficiently intermixed by mechanical agitation, the volume would be less than two pints, because the smaller shot

would be placed within the interstices of the larger;—the whole weight would be obviously the same, but the specific gravity would be increased, proportionally as the volume is diminished.

In proposing such theory, the volume of a fluid is supposed to consist of separate atoms or particles, which being the usually received notion, renders a proof unnecessary, as it accords with fact and reason; and as the laws of gravity are subdued, or rendered void by the attraction, affinity or adhesion of water to acohol, so that, being intermixed, the lighter liquid does not ascend or become disunited from the heavier; and as such would appear rather a mechanical than a chemical action, the conclusion above arrived at seems fully established. The necessity for a distinction apparently minute and inconsequential, will be evident to such as may afterwards examine the subject of specific heat.

In selecting data whereon to establish a just thermometric scale, such authors will be quoted, whose authority appears bounded by plain common reason, at the same time that it administers to one common purpose.

First. Deluc (Chem. Dic., p. 277) considers the expansion of water, from the point of greatest density, to vary with equal amount, by an equal change of temperature, whether of increase or decrease.

Second. Sir Richard Phillips (Arts of Life, p.

663). "The expansion of water is the same, for any number of degrees, above or below the maximum of density," and he proves it by reference to an experiment by Dalton.

Third. Dr. Ure (p. 788). If the barometer differs from 30 by one inch, then the boiling point of water will differ by 1·92° F.; and when the barometer stands at 29 inches, water boils at 210·08° F.; and when it stands at 31 inches, the boiling temperature is 213·92°.

Fourth. Sir Richard Phillips (Million of facts, p. 441). "Elastic fluids expand equally, with equal increase of temperature."

Fifth. M. Gay Lussac. "The temperature of vapour, is that of the hot solution from which it arises."

On these data I say, that by Deluc, if water expands uniformly and equally by an equal change of temperature, above or below the point of its greatest density, that temperature and expansion are equal; and, as the force of water is proportional to its expansion, so is it also proportional to the temperature. Likewise, as water expands by an increase of temperature, above that at which it is at the point of maximum density; in the same degree that it expands by a diminution of temperature below that point, it follows—that the point of maximum density of water is that of zero; for on no other condition can a just scale of heat be geometrically constructed. Hence, temperature,

expansibility, and force, are equal. The quotation from Sir Richard Phillips confirms, in few words, the assertion of Deluc.

Sir Richard Phillips also says, that elastic fluids expand equally with equal increase of temperature, and M. Gay Lussac, that the temperature of vapour is that of the hot solution from which it arises, whence we infer, that the same law which governs the force, temperature, and expansibility of water, governs also that of its vapour or steam.

The quotation of Dr. Ure goes to show, that the temperature of boiling water, is proportional to the atmospheric pressure, at the time of its being boiled; so that, viewing the whole subject at once, the following theory appears to be established;—that the force of water at any temperature, including that of its boiling point, under ordinary atmospheric pressure, or under increased pressure as that of the force of steam confined and incumbent over it, and also that the force of steam under equal and like circumstances, is proportional and equal to the temperature.

We will not stay to enquire, what objection might be raised against such theory; but if any valid obstacle should present itself, it shall have full consideration as the subject advances. It is however satisfactory to be enabled to say, the theory accords with my own observations made some forty years since, that by an uniform degree of heat, the force of steam confined within a boiler, increases

uniformly and equally in equal times; and that, as the force is the result of equal increments of heat, so are force, expansibility, and temperature, each equal to the other.

Before describing the principle of the intended scale, it is proper to say, that it will be termed the Steam Scale, on account of its being designed expressly to represent the relative temperature of steam at any pressure, of which the volume in water is one, and the pressure the same. Such scale being designed for one expressed purpose, its applicability to other uses, as that of determining the specific heat of different bodies, will be disregarded; because it would involve considerations, perfectly distinct from those contemplated, such as specific gravity, or chemical action.

In the following figure, the expansion of mer-

cury is supposed to be employed, as the measure of the temperature, &c., of steam; and on the line FB, let A, the point whereat F B intersects D C, be taken as the point of maximum density of water; let A B = the expansion of mercury by the same increase of temperature, that would expand one volume of water under a mean atmospheric pres-

sure, equal to B C, so that B C is the expansion of water from the point of its maximum density, to its volume at the steam or boiling point at the said pressure.

Also let A F = the contraction of mercury from the point of maximum density of water to the ice or freezing point. Let F D be parallel to B C, and F D represents the expansion of water within the prescribed limits.

The degrees afterwards to be described on the line A B are degrees of positive heat, and on A F degrees of negative heat; for water expands uniformly and equally by equal *increase* or *decrease* of temperature, from the point of its maximum density, and therefore the point at A is that of zero, as said above.

To carry out a scale on these principles, let the point of maximum density of water,—the expansion of water in volume under a given pressure on conversion into steam, as well as at the ice point be determined by experiment, relatively to the contraction and expansion of mercury within the same limits; and let A B be divided into so many parts, to be numbered progressively from A to B, as numerically represent the expansion of steam in volume, within the prescribed limits as found by experiment; and such numerals are degrees of positive temperature. From A in direction F set off as many points or parts, equi-distant as the degrees on A B, and numbering such points progressively

from A towards F, such numerals are degrees of negative temperature, and if there be a remainder, it is a fraction of the same.

Continue the line F B from both extremes to any desired length, on A B so continued, point off equal spaces to those on A B; continue the numbers progressively, and the numerals on such continued line, are degrees of positive temperature, of expansibility and of pressure;—for pressure and expansibility increase proportionally to temperature. On A F continued, and commencing at the greatest numeral (if a fraction of a degree shall have remained), continue the degrees the same as the last mentioned, and they are degrees of negative temperature.

The method of making the necessary experiments, in order to construct the proposed scale, so that the instrument be correct, deserves further explanation. It appears (Chem. Dic., p. 274), that the expansion of mercury in volume, within the prescribed limits varies, according to different experimenters, from $\frac{1}{50}$ to $\frac{1}{59}$, a difference which ought to be rectified, by future and more accurate observation. Of the experiment by Dalton, Dr. Ure observes, it is certainly too great, "being perhaps modified by his hypothetical notions." Such mere conjectural assertion will not be sufficient warranty for its withdrawal, in case of taking a mean of the eight experiments he records; for if we admit that of Dalton as in one extreme, is it not probable the

experiment by General Roy is as much in the other, for the mean of their experiments is $\frac{1}{54 \cdot 5}$,—the mean by Lord Charles Cavendish and Deluc, is the same, and that by all the others is $\frac{1}{54 \cdot 79}$. If then $\frac{1}{54 \cdot 5}$ be assumed as the expansion of mercury, the error may be inconsiderable.

In the preceding demonstration, the increase in volume has been necessarily taken; but mercury by its expansion in glass, in the thermometers now in use, indicates lineal increase (Chem. Dic., p. 787) so that $\frac{1}{63}$ is substituted for $\frac{1}{54 \cdot 5}$. Therefore, such scales, as indicators of relative expansion of water and mercury, err in this respect as 54·5 to 63. It is requisite in forming a thermometer, that the contents of its bulb be so proportioned to a section of its stem, as to represent the actual expansion of mercury in volume.

With respect to the method of reducing lineal expansion of solids, to that in volume, or *vice versâ*, Dr. Ure (pp. 272, 273) having given a table of lineal expansion in decimals and vulgar fractions, says, "to obtain the expansion in volume, multiply the above decimal quantities by 3, or divide the denominators of the vulgar fraction by 3, and the quotient in either case is the dilation sought." Here are two mistakes, first the rule is unintelligible, so let us presume the doctor intended to have said, *the product in one case and the quotient in the other;* and next it is erroneous for the cube of the expanded length less 1, is the expansion in

volume; because the unexpanded volume is 1. The difference of thre two rules of the expansion were 0·9 is as 2·3 to 6·859.

From what has been said respecting the two scales in use, as to the point of zero, their relative difference as to blood heat, in reference to the boiling point of water, and as they differ nearly as 24·39 to 5·4, in the comparative temperature of water at the point of its maximum density and its boiling point, and when we consider the error, arising from the cause above described; and the further consequence of such accumulation of error upon error; and when we also reflect, that the increase in volume of water, on conversion into steam, varies according to different experimentors as 1694 to 1800; we are constrained to admit, that all which has been done in this respect by chemical philosophers, has not given us one leg to stand upon.

It would increase these number of these pages far beyond what is desirable, were the precise mode particularised, whereby every experiment becomes of value; but the expansion of water on becoming steam, will be found as follows:—

Let there be a vessel made of copper, or other fit material, of which the inside is clean or bright, and in shape similar to that of a balloon or egg; and of which the capacity is known, when at a temperature, equal to that of steam under a pressure, of or equal to that of 30 in. of mercury,

and supposing such pressure equal to the mean pressure of the atmosphere.

Let the vessel be provided with two cocks, one fixed at its lesser end, and the other at the larger; and so that they be nearly in the same line, and let its strength be such, as will resist the maximum atmospheric pressure, when a vacuum is formed within.

Let the vessel, after having been filled with water, be placed within an enclosed boiler; so that the smaller end being downwards, the cocks will be nearly perpendicular; and let there be a means of opening and shutting them at pleasure, from without the boiler; and let the uppermost be opened and the lower closed.

Next, let the boiler be entirely filled with water, and let it be furnished with a valve placed at its summit, and so loaded, that a fire being made under the boiler, the valve will open, when the pressure against it is equal to that of 30 in. of mercury.

When the water within the boiler shall have been discharged and evaporated, so that its upper surface be so much below the lower end of the lowest cock, as to allow space for the water remaining in the internal vessel, if any, to be added to that in the boiler; and its surface after the addition somewhat below the low end of the lower cock, let the latter be gradually opened from without, so that the water

which may remain in the inner vessel be discharged into the outer, which is the boiler.

A short time afterwards, or when the inner vessel shall be filled with steam, let the two cocks be closed; and when such steam shall have been condensed into a liquid, let its volume be determined, when its temperature is that of water at a maximum density; and the capacity of the vessel as previously determined, is to that of the water as the term of expansibility of water on conversion into steam, under the aforesaid pressure, and within the prescribed limitations of temperature, and which are from that of the maximum density to the boiling point of water.

If for philosophic purposes, it be necessary to carry out a table of expansion to seven places of decimals, or to the ten millionth part of 1, as appears in the chemical dictionary, distilled water should be used in the experiments; and as the boiling point of sea water, differs from that of pure water, similar experiments thereon should be made; both to establish the validity of the steam scale, and to determine the relative advantage or otherwise, of salt water for the purpose of steam navigation.

To render this book useful to all who may take occasion to consult it, it is desirable to give examples of the method of using the steam scale. Such method will vary according to the end de-

sired;—the scales now in use, are supposed to measure both temperature and volume, as will be obvious when examining the theory of latent heat; it is therefore proper to say, the steam scale is applicable to atomic temperature only, and that temperature in volume is obtained as follows:—

Let us suppose the expansion of water under the aforesaid pressure, on conversion into steam to be found by experiment 1728;—or that 1 inch of water forms 1728 inches of steam.

Then suppose the atomic temperature of blood were required on the steam scale, the boiling point by Fahrenheit is 212°, and blood heat 98°, and deducting 40° from each, as the error in fixing the point of zero; boiling water will be to blood heat as 172 to 58. And as 172 : 58 :: 1728 : 582·7—the atomic temperature of blood by the steam scale.

Next suppose the temperature of blood to be required in volume:—

$\frac{172}{58} = 2{\cdot}9655$, and $\frac{1728}{\sqrt{2{\cdot}9655}} = 1003{\cdot}4$ blood heat in volume by the steam scale.

The degree on the steam scale corresponding to 74° Fah., is $\frac{1728}{\sqrt{\frac{212-40}{74-40}}} = 768°$.

Suppose it be now required to find how many inches of water at 74° Fah., or 768° of the steam scale could be heated to the boiling point 1728°,

S.S.; by a cubic foot of steam under a pressure $=$ 30 in. of mercury. Then

$768^2 = 589824$, the volume of heat in 1 in. of water at 768°, S.S., and $\frac{1728^2}{589824} = 5\cdot06$ cubic inches that would be raised to the boiling point.

The experiment of Mr. Watt goes to show, that 6 in. of water were heated from the atmospheric temperature to the boiling point, by a cubic foot of steam, and a residue left of 900°, termed latent heat. The difference between 5·06 and 6 in. is very nearly accounted for, by his having employed an instrument erring as 63 to 54·5; therefore, by the steam scale only about 5 in. would be so raised in temperature, that is when the error in the usual thermometer as to the expansion of mercury in glass has not been rectified, but of the 900° of latent heat we will institute further enquiry.

The temperature of the 6 in. of water employed in Mr. Watt's experiment is not given, excepting that it was of that of the atmosphere, which we will suppose 74° Fah., deducting which from 212°, it appears each inch would require to be raised 138°, and the six inches would therefore absorb 828°. The number of inches of steam employed is 1728, from which deducting 828°, there is a remainder of 900°, which it appears are termed latent heat.

If such mode of culculation were made use of by Mr. Watt, to determine latent heat, and I do not perceive any other, it is not a mark of the

sagacity (Chem. Dic., p. 289) ascribed to the discoverer; but rather of obtuseness, scarcely to be equalled, excepting in the course of mathematics by Dr. Hutton. If Fahrenheit's thermometer be a measure of temperature only, and not of volume, which cannot be denied, the calculation preceding should have been thus,—1728 inches of steam at 212°—the whole temperature is $1728 \times 212° =$ 366,336 degrees, contained in a cubic foot of steam; —from which deduct (as shown above) 828 degrees, and the remainder is 365,508 degrees of latent heat, instead of 900°.

By a process so manifestly absurd, applied to a scale so absurd, it is seen how error accumulates. Had Mr. Watt proceeded as follows, it would have been an instance of the sagacity imputed to him, $\frac{212-40}{174-40} = 5{\cdot}06$ nearly, would be the inches of water heated as aforesaid, and nothing remains for latent heat. It was said of old in the nursery, " A man made a mouse-trap, but he could not set it,"—a facsimile of the operation just reviewed.

The last process will suggest, that it may be desirable to graduate a thermometer by different scales, according to the purpose for which it may be used; or otherwise that it be accompanied with a table, corresponding to its degrees, showing atomic temperature, the same in volume, force as applicable to steam in reference to any standard, the quantity of water requisite to form a given power of steam, as well as to condense it, and for other pur-

poses, as the chemical philosopher may find useful in his researches.

As to freezing mixtures,(Chem. Dic. p. 835) snow and sulphuric acid sink Fahrenheit to 91 degrees below zero, and blood-heat is 98° above—the difference then is 189°. The difference of blood-heat, and the boiling point is only 114; would not then cold produced by 91° below zero freeze greatly more in degree than boiling water would scald, a conception so improbable that the doctor must have placed very implicit faith in his instrument, ere he would venture to put it in print?!!

In the Chemical Dictionary (p. 289) we read, in proportion as the sensible heat augments, the latent or specific heat diminishes—it may then be inquired, what is to be said of sensible heat when, by a just thermometer, the term latent heat has been banished?—and further, need there be any cause for alarm, that chemical philosophers have failed in their attempts, to discover some laws in the specific heat of bodies?

These matters are put before the reader as interrogatories, to avoid the necessity of a proof, which would be less instructive than tedious; for he may have noticed an assertion has seldom been made, unattended by a demonstration, or otherwise showing sufficient cause.

To conclude the subject, it may be said, if the two scales now in general use be good they would agree,—they differ, consequently, one at least is

bad; the rule is also bad whereby latent heat has been discovered, if temperature be any thing beyond mere fictitious imaginary quality, or accidental property.

Let us, however, conceive it to possess relative proportion, so that it be comparable to something either essential or material, as alcohol or salt; and that each cubic inch of steam absorbing the one or the other be represented by T; and so that T becomes a supposititious term for temperature or quantity of heat, alcohol or salt, in each cubic inch of steam.

Then the quantity of alcohol or salt, contained in the volume of steam, the produce of 1 cubic inch of water is 1728 T, and that quantity divided by 6 will give 288 T, infused into each cubic inch of water,—so there remains neither latent alcohol nor salt; for the steam has lost in volume of alcohol or salt, precisely what the water has gained. And hence by the premises, if temperature be not mere fiction, latent heat is absolute folly, and both scales now in use false.

Furthermore, let a given portion of steam be condensed mechanically into any less volume, and as the volume is diminished, so are the force and temperature proportionally increased. Thus let the ratio of condensation be as 2 to 1, and the same quantity of heat which, before compression, was contained in 2 cubic inches, will, after compression, be contained in 1;—the space occupied by each

atom being diminished one-half, its temperature is doubled, and as the number of atoms before and after compression are equal, the temperature of the whole is increased proportionally as the volume is diminished, and, therefore, the steam scale is unerring, whether the temperature be generated by the absorption of heat, or by compression.

This theory is founded on the assumption that heat is matter, for matter occupies space, and so does heat, and the space it occupies (under given pressure), and the number of particles contained in that space of steam is determinate by observation.

That pressure is increased in the same ratio as the temperature is so evident, that a further demonstration would be a work of supererogation.

Lastly, the steam scale has been projected in accordance with certain data,—and such data have been proved hypostatic:—the afterproof having confirmed the data, the scale is established on unerring principles;—theory and practice obviously and necessarily are in agreement; and, at the same time, as distinct as the first letter in the alphabet from the last.

For the convenience of whoever may construct a thermometer on the principles of the steam scale, the following hypothesis are submitted for examination:—

Let $S =$ the expansion of water in volume on conversion into steam under the atmospheric pressure.

Let g = the relative gravity of mercury to water.
p = the pressure of mercury in lineal inches.
Then $\frac{s}{p}$ = the expansion of mercury in volume.
g = and $\sqrt{s} \times g$ = the boiling point of mercury.

THE EXPANSIVE STEAM ENGINE.

Its name originates thus,—the supply of steam from the boiler to the cylinder, is cut off at any desired portion of the stroke of the piston; and by figures on paper, it appears increased power is obtained, in consequence of its dilation; into which it is my present purpose to enquire.

By the thermometric scale described above, pressure and temperature are equal,—their equality is as essential to the constitution of steam, as is a certain proportion of the elementary substances, forming water or air. If this be doubted, let a vessel containing steam be provided with a mercurial gauge to show the pressure; and a thermometer on the steam scale to indicate the temperature; and as the one falls so will the other, a small allowance in time being made for the escapement of heat contained in the glass of the thermometer.

Supposing this to have been proved, it follows, that if a vessel, being of copper or iron, the tem-

perature of which is less than that of the steam to be admitted into it, the pressure of the steam cannot act on the vessel, or on the piston, if we suppose the vessel to be the actual cylinder of a steam engine, until the cylinder and its piston be raised in temperature, equal to that of the steam within it, the proofs of which are various, and as follow :—

First, Theoretically the constitution of steam is water and heat,—the latter is abstracted from the steam, and absorbed by the cylinder and piston, until their temperature be raised equal to that of the steam; and during such abstraction, absorbtion, and consequent condensation of the steam, the water contained in the steam is being disengaged, during which time and whereby the force of the steam is sacrificed.

However obviously correct such theory may be, I purpose to test it by actual experience; and by reference to an experiment by the "ingenious philosopher" (Chem. Dic., p. 289) M. Clement, and by an hypothesis propounded by Mr. Watt aforesaid.

The experiment of the first-named goes to show, that when high pressure steam acts on the safety valve of the boiler, a vacuum is, in certain cases, formed between the valve and the lid, so that the boiler may burst although the steam be escaping. The last fact is admitted, but not so the vacuum, because it is in opposition to all experience, for, could we suppose a vacuum, it would be instantly filled, either by steam from within the boiler, or by

the air from without. We then infer there is an absolute plenum; and, although the steam be in conjunction with the lid of the valve, it does not exert its pressure against it, because the supply of steam is not sufficient to raise the temperature of the lid, to its own temperature, and hence it is proved that the steam within a cylinder cannot exert its force against the piston, until both are of one temperature.

Of Mr. Watt's three capital improvements (Ch. Dic., p. 292) which, according to Dr. Ure, seem to have nearly exhausted the resources of science and art, of the first he observes, " On opening a valve, or stop-cock of communication, the elastic steam which had floated the ponderous piston, rushed into the distant chest," (the condenser) " leaving an almost perfect vacuum in the cylinder."

On this I say, the effect produced is the same as if an almost perfectly vacuum were formed; nevertheless, such vacuum is not formed in the cylinder; for first, the steam rushes out of the cylinder just as fast as it rushes in, so that the rush is distinctly heard, through the exhausting valve, during nearly the entire time of each half stroke of the engine. In the condenser, it comes in contact with the injection water, and continues in such contact, during the entire of each half stroke; but, as the injection water is of less temperature than the steam, although there be an absolute plenum in the cylinder and condenser it does not resist

against the progress of the piston. This, however, admits of a modified explanation; there would be an absolute plenum were a partial vacuum not created by the air pump; but if no air be contained in steam, and the cylinder and all which communicates with it perfectly air-tight, Mr. Watt's engine would work with equal effect, in case the air pump were only of the same diameter as the cold water pumps, and with an increased length of stroke, as $1\frac{1}{4}$ to 1, for it has been proved, that the steam when in contact with the injection water, does not make any resistance to the progress of the ponderous piston; but in as much as the vacuum has been increased, and additional force gained in the cylinder from the action of the air-pump, in so much is the steam and its power expended in creating that action.

The air-pump then is but a necessary evil, to remove the air which may find its way into the cylinder or condenser.

It is next to be explained, that, although the steam does not exert its force, that is, its full force against the cylinder or piston; it may nevertheless exert a portion of such force,—for example; let it be supposed that in any cylinder the steam be cut off at one-sixth of the stroke of the piston, it will consequently, when the full stroke has been made, have expanded six times,—if the steam when admitted be of a pressure equal to that of 30 inches mercury, its atomic temperature would be 1,728°,

but when it has expanded six times, it would be only $\frac{1728}{6} = 288°$ by the steam scale. Let us suppose the stroke of the engine to be performed slowly, and that the metal composing the cylinder is not possessed of the property of conducting heat, so as to produce a uniformity of temperature throughout its length, and so that, when such stroke shall have been completed, the temperature of the cylinder at one end would be 1,728°, and at the other 288°. Then on the return of the piston, the steam being at 1,728°, will be in contact with the cylinder at only 288°,—the cylinder will exhaust the steam of its temperature, and consequently of its force, in such degree, that whilst the steam employed is of the force and temperature of 1,728°, its effect on the piston is only that of 288°. Consequently, five-sixths of the force of the steam is thus sacrificed or wasted. Such degree of waste however will diminish progressively as the piston advances, because the temperature of the steam will diminish after the steam shall have been cut off, and the temperature of the cylinder will increase, so that at half stroke their respective temperatures would be equal.

During the first half of the stroke then, we perceive that the heat has been abstracted from the steam, and the result is a deposit of water, either on the piston or on the cylinder bottom, according as the piston may be making the upward or down-

ward stroke, and which water must either be again converted into steam or discharged through the exhausting valve, so that it may find its way to the condenser, and thence to the air-pump. It is unnecessary to examine which actually takes place—for, as regards force, in connection with temperature, the consequence is the same, but, as regards force, in reference to effect it would differ in a small degree.

In such view of the case, that is, when the velocity of the piston is supposed to be slow, and iron a non-conductor of heat, we see a large portion of heat misemployed; first, by condensation during the first half stroke of the engine; and during the remaining half it is employed, though in a partial degree, in the re-conversion of the water into steam;—but, admitting iron to be a conductor of heat and the velocity of the piston at the usual rate, the effect produced is the same as described; for the temperature of the cylinder will be uniform, it will be a mean between 1,728 and 288, or rather somewhat above it; and if we take into consideration, the external condensation occasioned by the atmosphere, on the enlarged surface of the cylinder, its top and bottom, consequent on its increase of diameter to adapt it to the expansive principle, the inference is, that figures fail in practice, and the expansive principle, in the manner in which it is being carried out, an is idle story.

The expansive principle, as at present put in practice, has failed, and it is now time to consider the reason. It is 46 years since I heard from the lips of Mr .A. Woolfe, that, to ensure its success, it was requisite that the steam cylinder be encased within a jacket, and the interstice filled with steam of the pressure of that before being expanded;—and why so, is a question which need not now be answered. Bolton and Watt also enclosed the cylinder of the condensing engine in a jacket during the time of the patent, and whilst the patent fee was proportioned to the fuel saved;—we need not ask why; it is sufficient to observe, that of late years, the jacket has been generally discarded.

Being aware of the evil of illustrating mechanical principles by analogy, ever since the trio of mechanics, mentioned above, compared a double engine to two men turning a grindstone, it will only be said,—that ordinary steam is three times as sensible of change of temperature, as is the human body; and let the cylinder be clothed accordingly.

It still remains to be said, that in order to discover the full merits of the expansive principle, it would be desirable to erect an engine of four or six horse power, so planned that it might be used with the jacket or without. There is one and only one purpose to which it can be applied, so as in a simple way, to settle the question of utility or ad-

vantage,—but there will be an expense incurred—perhaps of five or six hundred pounds, and who is to pay it? It is submitted that it be paid by the public, for whose benefit it would be intended; but if it be objected to, let the "pound foolish" practice continue, and the "penny wise" laugh at it.

THE GREAT BRITAIN IRON STEAM SHIP,

And the Expansive Principle.

FROM the particulars of the above vessel furnished by Richard Guppy, Esq. as they appear in the *Mechanic's Magazine*, vol. 43, p. 151, the following are selected:—

Length of keel, 289 ft.—length aloft, 322 ft. main breadth, 50 ft. 6 in.—depth of hold, 32 ft. 6 in.—weight of engines exclusive of water, 520 tons.

The engine is of four cylinders, on the expansive principle, each 88 in. diameter and of 6 ft. stroke. The large diameter of the steam cylinders, was purposely with a view of working them very expansively; and on the trial recorded, the steam being at 4 lbs. pressure in the boiler, was throttled

on its passage, and cut off by the expansion valve at $\frac{1}{6}$th of the stroke; that is, 1 foot from the commencement, and the vacuum equal 26 in. of mercury. The vessel is moved by a screw propeller.

The boilers consist of one outside casing, 34 ft. long by 31 ft. wide, and 21 ft. 8 in. high; and this is divided into three distinct boilers, by means of two longitudinal partitions.

In each boiler there are four furnaces, four at the after, and four at the forward end, and therefore there are 24 fires in the whole.

On the journey from Bristol to London, the average speed was $9\frac{1}{2}$ knots, the pressure of the steam in the boilers varied from 2 to 5 lbs., and it was cut off at one-sixth the stroke, and the throttle valves were kept more than half closed.

The consumption of fuel was estimated at about 40 tons in 24 hours.

The power of such engine, the boiler excepted, taking 16 square inches of the section of the cylinder as that of one horse, would be $\left(\frac{88^2 \times \cdot 7854 \times 4}{16} =\right)$ 1520 horses, if worked non-expansively and by steam of the usual force, or $3\frac{1}{2}$ lbs. above the pressure of the atmosphere.

The power of the steam, as shown by figures when worked expansively, and the supply of steam cut off at one-sixth of the stroke, and under the supposition that no condensation is caused, as described in the last article, would be equal 32179877

lbs. $\times$ 1 foot, or that of 975 horses without any allowance for friction; and taking the pressure of steam at 15 lbs. per inch,—velocity of piston, 216 ft. per minute (18×12)—area of cylinders, 24328·5 in. as shown thus:—

$$\frac{\frac{24328{\cdot}5 \times 15 \times 216}{6} \times \sqrt{6}}{33000} = 975 \text{ horses, and if from}$$

which be deducted one-third for friction, there remains 650 horse power, without allowance for condensation, and supposing that the throttle valve be entirely open.

Had the engine been the usual condensing engine—taking (only) the same pressure of steam and allowing one-third for friction its power would have been $88^2 \times 4 \times {\cdot}7854 \times 216 = 78824340$, deducting one-third, leaves 52549560, which being divided by 33,000 is 1592 horses.

It may now be observed that the pressure of steam is as its expansibility, and its force during expansion proportional to its expansion. Also the force of steam is as the square of its expansibility, and therefore when the steam is employed expansively in the cylinder of a steam engine, the force is as the square root of the expansion.

On this rationale the preceding estimate has been formed, and I am prepared to say it is correct, however it may differ from that of Professor Davies, in the *Mech. Mag.* vol. 47, p. 31, made on the meagre data " that the pressure of steam being

inversely as the space occupied we have the pressure on the piston, &c."

Should he question the rule above given, he will find me ready for the public benefit to contend the point,—at present I will only say,—preserve me from the school of Hutton on mechanical subjects, and let not his followers pretend "the integral calculus becomes absolutely necessary."—(*See* Note A.)

We will now examine the effect produced by the engine of the Great Britain, which is as, and equal to, the resistance overcome. This examination will be made by a comparison with Her Majesty's sloop of war, Inflexible; as to which it should be noticed the latter is impelled by a paddle, and the Great Britain by a screw. The advocates of the former may consider the comparison unfavourable to the merits of the engine of the Great Britain. Whatever difference there may be in this respect will be neglected, and the cause will be evident when their respective merits come under observation, nevertheless a wholesale allowance will be made, which will amply compensate for the superiority supposed by its partisans, of the paddle over the screw.

We will first compare the respective resistance of each vessel on the following theory; viz., that after a vessel be under full way, the resistance of the water to its progress, is directly as the section of the vessel at midships, and inversely as its length.

Great Britain :—

Breadth 50·5 ft. × draught, about 13 ft. = 656·5 / Length of keel . . . 289 — gives the resistance of 2·27.

Inflexible :—

Breadth 42·0 ft. × draught, about 15 ft. = 630 / Length of keel . . . 190 — gives the resistance as 3·3

The resistance then to the Inflexible, is nearly half as much more as to the Great Britain; but as the theory may by some be disputed, and as it will be in favour of the Great Britain's engines, we will suppose the resistance to both vessels the same.

The speed of the Great Britain, on the passage from Bristol to London, was $9\frac{1}{2}$ knots per hour,—and if knots be nautical miles (I believe) $9\frac{1}{2}$ knots are equal to near 11 standard miles.

The maximum speed of the Inflexible, is $12\frac{1}{2}$ miles an hour, and the power required to overcome the resistance is as $12{\cdot}5^3$ to 11^3, or as 1953 to 1331. The resistance to the Inflexible, is then nearly half as much more than to the Great Britain; but to allow for any error as to the absolute speed, were the two vessels tried together, we will suppose the speed and resistance equal.

I have then granted in favour of the Great Britain, a bonus of $1{\cdot}5^2 = 2{\cdot}25$ per 1, or 225 per cent.

The power of the engine of the Inflexible is said to be 375 horses,—the engine has two cylinders, each 72 in. diameter, stroke 5 ft. 9 in., and 18 to 20 revolutions per minute, and consumes about 13 tons of coal per day.

Whence we arrive at this concluson, that of the engine of the Great Britain,—its dead weight and the cost are that of 1500 horses—its effective power not exceeding 375 horses, and that its consumption of fuel is three times ($\frac{40}{13}$) that of the Inflexible, but which we will reduce to only twice.

Both engines are on the expansive principle, but that of the Inflexible is thrown out of use,—the proportion of boiler power of the Great Britain to her cylinders, is such that she cannot afford to do the same. Had the Great Britain's engines been compared with a well proportioned usual condensing engine, its power would have been found not to exceed 300 horses. To what cause then is the defect to be ascribed, unless to the theory above submitted; and until it be overturned by the substitution of a better, is it not desirable the advocates of the expansive principle, should stay their hands in practical works, and merely report progress on paper?

If any practical mechanic should still desire further proof, of the inefficiency of power of the engine of the Great Britain,—let him note, that when the piston is at half stroke, the pressure of the steam is 5 lbs. on the square inch, supposing it but slightly throttled, deducting from which the friction of the enormous machine per square inch, let him say what is the effective force remaining!

STEAM NAVIGATION.

ITS PRINCIPLES.

DEFINITIONS.

1. Water being at rest, offers a resistance to the body, which causes its motion; which resistance, if considered irrespective of time and space, will be termed the module of resistance; and when considered in respect to both, it will be termed the whole resistance during a given time or on passing a given space, as the case may be.

2. The power requisite to overcome any resistance, is equal to the resistance; and similar terms will be used in similar cases, in respect to power as in respect to resistance.

3. Power may be considered either actual or nominal; actual power is the force exerted in passing any given distance, or during a given time, and nominal power is that exerted during any indeterminate time, or in passing an indeterminate distance, or as the power exerted during any or every particle of any indeterminate time.

4. The matter of the steam-engine is the nominal power, or the passive matter on which the power acts,—the actual power is the steam, which, by its action on the passive matter, produces motion ; and it is equal to the volume multiplied by the force pressure or temperature.

5. For the sake of uniformity with established custom, which is to measure power in lbs. × feet per minute or per second, the temperature, as shown by the steam scale described above, will be taken as reduced to pressure in lbs. per superficial inch.

PROPOSITIONS.

1. THE nominal power requisite to raise any weight at any uniform velocity, is as the velocity with which the weight is raised, or it is in the compound proportion of the quantity of matter and velocity in respect to time,

For by Emerson's Principles of Mechanics, Sect. 1, Prop. 2, " The quantity of motion is the sum of all the products of every particle of matter multiplied by its respective velocity."

2. The actual power requisite to raise any weight at any uniform velocity, is as the height to which it is raised, whatever be the velocity in respect to time and space.

For if the velocity be increased, the time of action of the nominal power will be proportionally decreased, therefore, whatever be the velocity, the actual power requisite to raise a given weight is as the height which it is raised.

3. The actual power requisite to overcome the resistance of still water to a body moving through it in a straight line is, as the cube of the velocity in equal times, and as the square in equal distances.

For the demonstration, see p. 32.

4. The actual power requisite to produce any velocity of a vessel through still water in a straight line, is equal to twice the actual resistance of the water to the vessel.

For the whole resistance of the water to the vessel in one direction in a given distance, is equal to the whole resistance of the water to the paddles in the opposite direction, and as both resistances are overcome by the actual power of the steam, a power of 2 of the steam is requisite to overcome an actual resistance of 1 to the vessel:—that is, if the power of 1 horse working on land, would draw a vessel through the water at any velocity, it

would require a power of 2 horses (if employed to move the vessel by paddles or the screw) to produce the same velocity. That is, supposing their power exerted in the line of motion of the vessel.

5. If an engine, when exerting its full power on paddle boards, considered as infinitely large in area, can produce in a vessel a certain velocity, and if the same vessel when propelled by sails, also infinitely large, were moved at the same velocity as when moved by the engine, the speed of the vessel, when both powers act together, that is the steam to move the paddles, and the wind against the sails, would be the same as when each power acts separately.

For let it be supposed the vessel is in motion by the power of the wind, and the steam being afterwards applied, it would require all its power to move the paddles at the velocity with which the vessel is moved by the sails:—but if by the reduction of the area of each paddle board one-half, or by increasing their velocity, so that their speed as regards the strokes of the engine be doubled, the speed of the vessel, when the wind and the steam act in conjunction, would be increased in the proportion of 1 to the square root of $1\frac{1}{2}$.

If the surface of the sails and paddles be supposed infinitely small, when both powers act together, the speed of the vessel would be as 1 to

the cube root of 2. Whence it follows generally, that whatever be the proportion of the sails or paddles to each other, or the speed of the vessel; when the actual powers of the wind and of the steam are equal, the speed of the vessel will be increased at some rate between 1 and the square root of 2.

N.B.—If it be objected that the supposition whence the conclusions are drawn is an impossible case, the answer is, that I already know it.

6. The fuel consumed by an engine is as the cube of the velocity of the vessel when the time is given, and as the square of the velocity when the distance is given.

For the consumption of fuel is as the actual power, (as per Prop. 3,) when the distance is given, and proportional to the nominal power when the time is given.

7. The velocity of the paddle exceeds that of the vessel.

For the vessel is propelled by the displacement of water in direction opposite to that of the vessel, and the water displaced is proportionally to the difference of velocity of the paddles and the vessel.

8. The module of resistance of the water to the paddles multiplied by their velocity, is equal

to the module of resistance of the water to the vessel multiplied by its velocity,

For the whole resistance is equal to the whole power.

9. The actual power requisite to move a vessel any given distance, is equal to the module of resistance of the water to the vessel multiplied by twice the distance.

For the whole resistance is equal to the whole power.

STEAM NAVIGATION.

ITS PRACTICE.

The principles of steam navigation have been given distinctly from its practical rules, and in the form of propositions, for the convenience of a numerical reference,—a short proof has been appended, which, although not always demonstrative, ought to suffice.

They differ so widely from the usual notions of mechanics both in England and America, that some notice of the causes becomes necessary. Although in the establishment of truth the mind ceases not its energy, the detection of error so long continued, becomes absolute nausea, and this must stand as an excuse for a curtailment of such proofs as may follow, should they appear incomplete.

Professor Renwick, of America, (Mech. Mag., July, 1846, p. 16) has written thus: "It is a fact ascertained from universal experience, that, in absolute contradiction of all theory, every foot which can be added to the velocity of the paddle, will give an additional foot to the speed of the vessel."

Here we have theory pitted against experiment,

and the latter it seems has borne off the trophy. I say, if an additional foot to the speed of the paddles gives the same increase of velocity to the vessel, the whole resistance in America is not *as* the cube of the velocity nor *as* its square, but *as* the velocity. If universal experience has proved that which has been asserted, the inference is, that whatever be the velocity of any vessel, the difference of the velocity of the vessel and paddles, commonly called the slip, will in all cases be the same, but in the Mech. Mag. (v. 41, p. 358), when the vessel is going 3·82 miles an hour, the slip is ·27 miles, but at 9·96 miles, instead of being the same, it is 2·4, or nearly nine times as much. Hence, although the experiments referred to are far from correct, they are sufficient to show the error of the professor's assertion.

Experiments then are to be received with caution; many a sound theory has been rejected, because the experiment to elucidate it was made on unsound principles, or the result not sufficiently digested; for in fact experiment, as may have been said above, often gives only apparent results, whilst that which is true lurks behind, and is seldom brought to bear but by the exercise of the reasoning faculty.

The after reasoning of the professor and his recommendation of the sun and planet motion is too childish to deserve comment; it is sufficient, the latter had a fair trial by the first practical

mechanic of his day, that, after being bolstered up on both sides, the cause of failure was evident, and it died a natural death about 40 years since. There may, however, be still seen the sun and planet doing small duty in summer time at Barclay's brewery, for so says Joe (the doctor of solids) on board this ship.

English engineers have a notion, as is evident from their scrips in the periodical above quoted, that the less the slip of the paddles, the more effective is the power of the engine. One writer therein says, he has entirely removed it, but the assertion was not of sufficient weight to induce me to take a note of his name or the page. The slip of the paddles is a cant term for the difference between their velocity and that of the vessel;—then if Prop. 7 be established, he has beaten Professor Renwick out of the field, for the vessel described by the writer was moved not by the power of steam, but by an invisible magician. Let then the slip of the paddles be excused from further attendance in the study of the mechanic, and its place supplied by some other more expressive, but less seductive term.

The screw and the paddle have each their partisans, and their respective merits have been discussed, as if either the one or other had taken stable ground, whereon to base their reasoning. Neither have done so, for if it be desirable to know the power of any machine, from the work it per-

forms, it is necessary to know the resistance overcome;—if an engine be employed in raising a body it is requisite its weight be known; if, in overcoming any resistance as that of water, it is requisite such resistance be known. The resistance of the water to a vessel can be established only by experiment, and none such appears to have been made.

The power of the engine has been determined by the dynomometer, but that is insufficient unless it were established, that its power is exerted in the line of, or parallel to the line of motion of the vessel. This is obviously not the case, and it will vary in degree, as the dip of the paddle boards into the water may be greater or less. Hence, to determine experimentally the actual resistance of the water, is as essential to sound engineering, as it would be essential, in case an engine were designed to raise any matter, to know and determine the actual weight of matter to be raised.

This may not have been considered necessary, in the mode in which the paddle or screw has been usually applied, nor in fact would it be so, if inconvenience did not arise from extending in an undue and uncertain proportion, the area of the paddle-board, or the palms of the screw, as appears by Prop. 4; but I say, that when the paddle is applied as it should be, unless the resistance of the water to the vessel be known, neither will the power of steam, requisite to give any desired velocity to the

vessel, be known, for in that case, or when the paddle is properly applied, the relative velocity of the paddles and vessel is determinate.

All experiments then to determine the respective merits of the paddle and screw, have left the question uncertain; and it may be sufficient to say, that in still water, supposing the power of steam and resistance equal, and the friction equal, (though the latter seems practically impossible,) the screw or paddle will surpass the other, as the power which moves it may be at or nearest a maximum, and motion produced in the water as above said.

Their respective merits in disturbed water will be according to the disturbance, and determinable only by experiment.

Recent experiments, as reported in a Sydney newspaper, were made on the Thames with the view of determining the question under examination. The vessels were tried both as to speed, and when coupled together and acting as tugs. The screw was predominant in both; but, although neither a partisan of one or the other as now applied, I say, that had the screw won in the race and lost in the tug, its merits would stand just as high as when having gained both.

To describe the mode of determining the resistance of the water to a vessel, and so that it be applicable to determine with sufficient accuracy, the resistance to any other vessel, would require more pages than the reader's patience would allow

him to examine;—so that all that will be said upon it is, that it must presume nothing, but prove all.

The letters of Mr. Hoseason, commander of the Inflexible, published in vols. 41 and 43 of the Mech. Mag., show him an opponent of the screw propellor, and as decided an advocate of the plain, simple, original paddle-wheel. His being in command of a steam vessel of war, is a guarantee that his engineering education has been the best the English school of mathematics could supply;—his standing out from his brother officers, in support of certain theories, is a mark of his desire to propagate those, which he has been taught to believe, are sound and incontrovertible; but if there be a case wherein "a little knowledge is a dangerous thing"—the case is that of mechanics.

Mr. Hoseason, in his letter to Sir Wm. Parker, (Mech. Mag., vol. 43, p. 263,) advocates the theory, that the resistance to a body passing through a fluid, is as the square of the body's velocity, and in a letter (p. 282) to Sir Graham Hammond he says, "I have certainly been desirous of establishing the theory of the squares on so broad a base, that it will not easily be shaken, for I feel, that by a careful consideration of this beautiful theory in all its ramifications, the greatest benefit will accrue to steam navigation." Had Mr. Hoseason, when afterwards attacked by an anonymous writer, under the signature of "Pressure not Puff," adhered to his expressed desire of supporting the theory of

the squares, and so that it might continue to the end of time, he would have done well; but by giving way on the first attack, it proved that both his opponent and himself, having acquired some knowledge of the thing disputed, had stopped far short of the extent of information, needful to fully understand or elucidate the case in dispute. The theory, both of the squares and the cubes, have been fully established above, and the distinction made, so that they will stand for ever. In these observations it is to be understood, that since neither party was professing to deliver theories propounded by themselves, but those of others; and, as neither had undertaken as practical engineers to wield the square and compasses ere he was qualified, such mistakes become uncensurable,—and would not have been noticed, were it not an instance of the defective engineering education at our academies or colleges.

With respect to the screw propeller, and the multitude of patterns or shapes in which it has been caused to figure by patent or otherwise, they remind me of pattern designers of ladies' garments, of which the newest fashion is always reckoned the best;—as soon as they show that the screw or paddle, being proportioned duly to the resistance of the vessel, produces motion of the water, the nearest in direction to line of motion of the vessel, that will be the best;—until that be proved, the different devices may remain where they are, to

mark the taste, but not the judgment of their respective inventors.

The vast friction attending the shaft of the screw propeller, is a drawback on its utility, probably exceeding the drawback on the paddle in consequence of the intermittant resistance. The consequence of such intermission is, that the force of each paddle board on striking the water, is perceptible throughout the vessel, unless it be very strongly trussed, and therefore, a considerable portion of the power of the steam is sacrificed,—for no effect is produced without cause.

It also appears by newspaper reports, that experiments have been made to enable the Lords of the Admiralty to judge of the fitness of iron as a material for war steamers, and that at each shot, an entire plate was carried away. The result of these experiments are unsatisfactory, because it has long been ascertained, that two pieces of iron being rivetted together, is as strong at the joint as in any other part, nor is such unaccountable, when we consider the *immense* gripe of a rivet after contraction, and that less than one-half of the iron of the plate is abstracted by the rivet hole. We then infer the vessel on which the experiments were made, was not put together in a workmanlike manner,—there might be a mal-proportion of the rivets to the thickness of the plates, their number might be insufficient, the head or clench of the rivet the same, or the material bad. The latter is most

probable, for, in that case, the rivets might be considered ruptured, as soon as each had by cooling contracted.

It is then the place of the engineer to provide a remedy, or to look out for an expedient, which I will propose as follows:—

Let the upper works of the vessel be encased, either within or without, by iron plates, leaving an interval of 4 or 5 inches, and enclosed at the top,—let there be valves placed at the two highest points of the same, so that when water is pumped into the interstice, the air will escape through such valves. Water may be considered as non-elastic by compression,—and if it be perfectly so, the plate struck by the cannon ball would not give way, unless it were the weakest; for, supposing the water described to be in contact with 600 plates, each plate would receive the effect of a ball, being the six-hundredth part of the actual weight fired; so that if the shot fired were 56 lbs., the proportion of momentum on each plate would be abont 1½ ounce. Allowing then a small degree of elasticity to water, might not the same result ensue.

An old steam engine boiler without holes, being filled with water and fired at, would, in a great measure, show the effect, if the experiments be properly conducted, for if the theory suggested be of any value, it would not give way at the point struck, but at the weakest, however remote therefrom.

The newspapers in New South Wales have reported as a prodigy of science, a scheme of raising the Great Britain steam ship by gunpowder. The scheme is practicable if there be water in the ocean, and gunpowder on land to effect it; but the science of the thing would be in determining the distance at which the powder should be placed, and the minimum quantity that would accomplish the purpose,—if effected otherwise, it would not be by science, but by a successful blunder.

Perhaps the vessel might be floated as follows: —Let there be a steam vessel of 100 horse power securely anchored at a sufficient distance, the paddle boards of which being removed, attach ropes or chains of sufficient strength, to the main axle of the engine, and which axle would be used, as is the capstan. Let such ropes or chains be also attached to the Great Britain, and the 100 horse engine set to work.

If we suppose its paddles twenty times the diameter of its axle, the force to move the Great Britain would be 40 fold as compared with its effect to move the vessel by its paddles, or the actual pull equal to a weight of about 18 tons, to resist which, the chains or ropes, and main axle of engine, as well as the anchorage, must be sufficient, and the motion would be about 80 feet per minute.

If a force of 18 tons, which would be equal to

the pull of 72 actual horses, each with a force of 5 cwt. be insufficient,—and, suppose 6 times that quantity be requisite, a pitch chain might be applied to work round the main axle, and into the cogs of a toothed wheel, which being proportioned in diameter to the main axle as 6 to 1, the pull would be increased to 108 tons, but the velocity diminished from 80 to about 13 feet per minute. The power of such engine, leaving velocity out of consideration, would be as 24,000 horses, as compared with an engine of 100 horses applied to paddles in the usual way. If a vessel were designed for such purpose, and provided with proper chains and anchors, its success in such cases would be certain;—the best method of reducing the speed to accomplish the purpose would probably be by a worm or endless screw.

The foregoing observations having been made as to what the present practice is, it remains to be said what it should be. This will be explained on a broad scale—and why? The answer is simple,—the profit on this book is not expected to leave the author one halfpenny for each mile he has travelled to publish it,—it will not pay his passage,—it will scarcely exceed the sum which he could earn in one day as a surveyor, before he had seen twenty years,—and it will not leave him more per year for the time expended in study, before being qualified to write it, than what he could earn as above in

less than one hour,—therefore, he intends to be paid value for the invention now to be described, or to retain it.

Let the resistance to the paddles of a steam vessel be uniform in every part of its revolution,—let it be in a line directly opposite to that of the vessel's motion, and the difference of velocity of the paddles and vessel in a certain proportion, and steam navigation through still water will be carried to the highest practicable perfection, and about one-half of the power now employed will be saved. Without amplifying the resultant advantages, I say,—the saving of fuel, by the application of such principles, would equal the entire cost of the engine in about two years, supposing it to be in use twelve hours out of twenty-four.

In comparing the proposed method, as to its effects in still water, with the usual paddle, it will be as about 2 to 1,—in disturbed water, the advantages will be great but indeterminate,—and as to general convenience, it may be taken as considerable, because the diameter of the paddle wheel, and the area of each paddle board, will be vastly reduced.

In comparing it with the screw in still water, the advantages would be the same as in comparison with the paddle, and in disturbed water, the advantages will, in every sense be equal to that of the screw, and its principal objections avoided.

The engineering multitude of the present day,

may suspect that as usual the inventor may have been content with a one sided view of the matter, but such has not been his usual route, as he never could perceive its convenience; and with respect to payment for the invention, which in a certain case will be offered to the British government, it will not be expected until the invention be actually complete on any desired scale; when, if the result should not be satisfactory, the price agreed on shall be subject to be pared down, to the value which a committee of parliament, after hearing evidence may fix upon it. The provision on the behalf of the inventor simply being, that he may see the evidence, and be allowed reasonable time for a rejoinder (if necessary) before the committee decides.

I conclude the subject by saying the ordinary paddle wheel now in use will be employed in the invention, and that the whole is of such simple order, that were a committee of six English professors of mathematics, and as many practical engineers, to sit over it for seven years I suspect they would rise from their seats, with as little knowledge of the why and wherefore, as any one of their number can, whilst this is being written, possibly possess, for both the essence and substance seem too minute even to attract observation.

NOTE A., PAGE 151.

When the matter was written to which this note has reference, I was under the delusion that the professor's theory, from the decorations with which it was introduced to the mechanic, was a new discovery; but on taking up the Millwright and Engineer's Pocket Companion, by Wm. Templeton, London, Simpkin and Marshall, 1833, the delusion vanished, for the result, by a simple intelligible rule therein, is precisely the same as that given by Mr. Davies,—so that his theory may be taken as a new edition of an error which has been in print for 14 years. As it is contemplated that this book will circulate not only at home but beyond the seas, the correct rule will be given below, and the professor spared the penalty of stooping for the glove or running the gauntlet.

Explanation. The magnitude of a sphere is as the cube of its diameter, and so of similar figures, from the octagon to the square, of which the magnitude is as the cube of a side.

Steam may be considered as divisible into particles, of which each are similar figures, and in form at some mean between the cube and sphere, and consequently subject to the same law as to

dimension of volume. Each particle of steam, when not under pressure, would increase in volume as the cube of its diameter or side, and so would the volume of any given number of particles. But under pressure, the volume would be as the mean geometrical proportion which is as the square, and on this rationale the steam scale has been projected, for when the expansion of mercury indicates a or b, the force of steam is expressed by a^2 or b^2.

If then it be admitted, that such scale has been established on sound mathematical reasoning, it would follow, that the force of steam on expanding in a cylinder is inversely, as the square root of the number of times it has expanded;—in which case its pressure at any point of expansion, instead of being represented by the hyperbolic curves, would be shown by a straight line.

As however the steam scale has not yet been established, or examined or proved, it is desirable to establish the theory independantly thereof.

Let the section of the vessel in which the steam expands be a square; then, as its external dimensions would continue the same during the expansion of the steam; its expansion therein would be represented in lineal measure. Consequently, if each cube, supposed to constitute a particle of steam, be expanded therein, its expansion would be exhibited in length of the containing vessel, the lineal expansion so exhibited would thereupon be

as the square of the absolute expansion of each cube, and the force of steam on being expanded, consequently, is as the square root of the lineal expansion in the exhibited cylinder.

The same may be shown in other words. Steam on being expanded, occupies a space proportional to the cube of the diameter of each particle of the volume, when not under pressure. The expansion of steam when under pressure is proportional to the pressure, and therefore the expansive force of steam under any pressure, is the mean geometrical proportion between any number and its cube, which is the square. Consequently, the expansive force of steam on being released from pressure, is as the square root of its expansion.

It is now to be observed, there would be a difficulty in proving this theory experimentally by means of steam, on account of its variation in temperature, whilst the cylinder, on its admission therein, would be of uniform temperature. But since atmospheric air is subject to the same law as to its expansion and force, as that of steam, and as its temperature is at a much less ratio, it would be desirable to employ it in any experiment designed for such purpose, and the mode of making it should be as follows. Let there be a cylinder of copper 2 inches diameter, and 14 inches long, to which is adjusted a piston, of which the external edge is of leather turned. To the cylinder let there be fitted a mercurial gauge, to denote the pressure within,

and the distance which the piston moves to produce that pressure, will determine, if the pressure be as the distance which it moves, or as its square.

The rule to estimate the expansive force of steam in the cylinder, according to the theory given above is as follows :—

Let p = the pressure in lbs. on the piston, supposing a vacuum on the opposite side, and that it moves without friction.

t = the number of times the steam expands after being cut off, or the number of lineal feet it expands, when the steam admitted at its unexpanded force is 1.

And $p \times \sqrt{t}$ = the force in the cylinder during each alternate motion of the piston, or during each half revolution of the engine.

As it has been my design throughout to render this book of practical use, I will say to Professor Davies, when again you have occasion to come into contact with a similar subject; first embrace the principle, then "Seize the bull by the horn, instead of barking at his heels."—P. P.

TO

THE BRITISH NATION.

My task is done, and now before you. As no man is judge in the cause which he defends, it is for an impartial public to pass sentence. The principles advanced, are as adverse to those commonly acknowledged by the engineering world, as is the use of reason to its abuse. The differences are not such as deserve to be termed the splitting of straws, being mostly as the square or cube, which it is hoped will lessen your labour in deciding which is right and which is wrong.

You will find I have followed but one preceptor;—to him then will be referred the censure which may actuate your pen, if against me; and in the other event, the only merit to be claimed is for the patient endurance of the drudgery, needful to reduce Emerson's principles of mechanics into practice. With him the difference is but in one iota, an evident misprint, or accidental omission of one word. Hutton, it seems, did not so pronounce it, fancied he had sprung a mine, and, yielding to the infatuation, sent out the *real* resistance of a fluid against a plane at rest, when in his eye, Emerson